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VARIORUM COLLECTED STUDIES SERIES

Domingo de Soto and the Early Galileo

William A. Wallace, O.P.

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Domingo de Soto and the Early Galileo

Essays on Intellectual History

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PREFACE

This volume contains fourteen essays, one of which was first published in 1968 and the remainder over a fifteen-year period extending from 1985 to 2000. The underlying theme of the volume is signaled in the title to the opening essay, "Domingo de Soto and the Iberian Roots of Galileo's Science". As a whole the essays raise the question whether Soto, a Spanish Dominican, could have played a role in a little known but very important discovery, that of the law of falling bodies.

The second essay, that of 1968, explains the context of Soto's unprecedented statement that falling motion is *uniformiter difformis*, a Latin expression equivalent to our law that falling motion is uniformly accelerated with respect to time. Soto first made this teaching available in the second edition of his questions on Aristotle's *Physics* in 1551, and it appeared in seven more editions of his writings down to 1613. Related to this, manuscript evidence has now become available to show that Galileo Galilei made his discovery of uniform acceleration at Padua shortly after 1604. He did not publish this teaching, however, until the *Two New Sciences* of 1638. The question thus arises whether Soto's view of acceleration in free fall was known to Galileo while he was at Padua in the first decade of the seventeenth century, and so might have been a factor in his discovery. The reference to the "early Galileo" in the title signals that the discovery was made before Galileo's work with the telescope and did not have to await his later writings. And the subtitle of the volume, "Essays on Intellectual History", calls the reader's attention to the fact that what is involved here is more the history of an idea, uniform acceleration in free fall, than it is the history of either of the two principals, Soto or Galileo.

The essays are divided into two parts. The first, entitled "Domingo de Soto and Sixteenth-Century Physics", contains seven essays, three of which provide textual studies of Soto's laws of motion while the remainder sketch the sixteenth-century context in which they were developed. In particular they detail the ways in which Soto's teachings were transmitted in Spain and Portugal to the early seventeenth century, mainly by Jesuit scholars. The second part, entitled "The Early Galileo on Laws of Motion", focuses on the young Galileo and his experimental work at Pisa and Padua, providing details of his regressive methodology and how it led to the law of uniform acceleration. Textual evidence is

presented for an indirect influence of Soto's work on Galileo, mediated by Jesuits who were teaching at Padua in the first decade of the seventeenth century.

The reader should be aware that the essays in the second part appear against the backdrop of many books and articles in which I have dealt with these and related topics. The most relevant is my earlier volume in the Variorum series entitled *Galileo, the Jesuits, and the Medieval Aristotle* (Ashgate, 1991). The essays in that collection should now be supplemented by two additional studies: "Galileo's Jesuit Connections and Their Influence on His Science", in *Jesuit Science and the Republic of Letters*, ed. Mordechai Feingold (M.I.T., 2003), and a derivative study, "Jesuit Influences on Galileo's Science", in *The Jesuits II: Cultures, Sciences, and the Arts, 1540–1773*, ed. John W. O'Malley et al. (Toronto, 2004). A fuller overview of the background of the essays is given in my *Galileo and His Sources* (Princeton, 1984). Two of Galileo's Latin manuscripts, composed around 1589–1590, provide the textual basis for the essays. The first of these I have translated in *Galileo's Logical Treatises* (Kluwer, 1992), for which I supply a commentary in *Galileo's Logic of Discovery and Proof* (Kluwer, 1992). The second is translated in *Galileo's Early Notebooks: The Physical Questions* (Notre Dame, 1977), for which I provide supporting documents in *Prelude to Galileo* (Kluwer, 1981). Fuller bibliographical details for these works are given in the notes to the essays.

With regard to the essays themselves, they do not have the same orientations as the books, though in some cases they anticipate materials to be found in them or further explicate their contents. Eight of the essays were written for presentation at symposia or conferences, while the remaining six were invited papers. They appear here in their original form, only slightly revised for publication. Their varied audiences and purposes required a fair amount of duplication, particularly in the second part where Jesuit influences on Galileo are discussed. I trust that these reprinted papers will be of use to readers who have not seen them in their original form as well as to those who already have. With the exception of the earliest essay (II), which appeared in abbreviated form in *Prelude to Galileo*, none has been previously reprinted.

Since the circumstances of the initial presentations are generally not preserved in the essays as published, a few details may render them more comprehensible for a general audience. Of those in the first part, essay I was presented at a conference entitled "Hispanic Philosophy in the Age of Discovery" held at The Catholic University of America in October 1992. The conference was occasioned by the commemoration of the five hundredth anniversary of Columbus's discovery of America. The 1968 paper, II, was read at a colloquium of the Department of the History of Science, Harvard University, in May 1967. A related essay, III, was an

invited contribution to a Festchrift honoring John E. Murdoch, of that department, on his seventieth birthday. The next presentation IV, which complements III with fuller details, was invited for a special issue of the *Revista Portuguese de Filosofia* celebrating the fiftieth anniversary of the founding of that journal. Essay V was invited for a special edition of the journal *Synthese* devoted to the history of science and its relation to philosophy. Then VI, like III, was an invited contribution to a Festschrift honoring Ralph McInerny, of the Department of Philosophy of the University of Notre Dame, on his seventieth birthday. And the final essay, VII, was read at the annual meeting of the History of Science Society held in Norwalk, Connecticut, in October 1983.

In the second part, VIII was presented at the Inaugural Conference of the University of Maryland's Center for Renaissance and Baroque Studies in March 1982 and devoted to the theme "Print and Culture in the Renaissance". Essay IX was read at an international conference held in Cracow, Poland, in May 1984, on the theme "The Galileo Affair: A Meeting of Faith and Science", The conference, itself an activity of a Commission appointed by Pope John Paul II to re-examine the Trial of Galileo, was sponsored jointly by the Cracow Pontifical Academy of Theology and the Vatican Astronomical Observatory. The next study, X, was written for a volume honoring Stillman Drake for his many contributions on Galileo and the history of science. (As a foreword to the volume, the editors published my citation extolling Drake as recipient of the Sarton Medal of the History of Science Society in December 1988.) Essay XI was invited for *The Cambridge Companion to Galileo*; similarly, XII was invited for a special issue of the journal *Vivarium* devoted to Medieval and Renaissance logic. The next, XIII, was delivered at a two-week postgraduate seminar treating Method in Sixteenth-Century Aristotle Commentaries, sponsored by the Foundation for Intellectual History, London, and held at the Herzog August Bibliothek in Wolfenbüttel, Germany, in September 1992. And the final essay, XIV, was given in Vico Equense, Italy, in June 1993, at a conference on Scientific Controversies sponsored by the Istituto Italiano per gli Studi Filosofici, of Naples, Italy.

Were I to rewrite these essays today they would probably differ considerably from the way they now appear. Nonetheless, I still stand by the theses they develop. The Addenda at the end of the volume serve mainly to bring bibliographical references up to date; references to these are indicated by an asterisk (*) in the margin beside the passage concerned. The Index of Names identifies the many authors referred to in the essays.

Let me take the opportunity to thank friends and colleagues of the Committee on History and Philosophy of Science, Department of Philosophy, University of Maryland, College Park, with whom I worked

from 1988 to 2003, a period during which many of the essays in this collection were written. Similarly, I wish to thank friends and colleagues at The Catholic University of America in the Schools of Philosophy and of Arts and Sciences, especially the Departments of History and English, for their encouragement and assistance over the past forty years. I am grateful also to the many persons who initiated the works or hosted the conferences in which the essays were presented and thus stimulated me to prepare them in the first place.

The Catholic University of America WILLIAM A. WALLACE
Washington, D.C.

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PUBLISHER'S NOTE

The articles in this volume, as in all others in the Variorum Collected Studies Series, have not been given a new, continuous pagination. In order to avoid confusion, and to facilitate their use where these same studies have been referred to elsewhere, the original pagination has been maintained wherever possible.

Each article has been given a Roman number in order of appearance, as listed in the Contents. This number is repeated on each page and is quoted in the index entries.

Corrections noted in the Addenda have been marked by an asterisk in the margin corresponding to the relevant text to be amended.

I

Domingo de Soto and the Iberian Roots of Galileo's Science

Visitors to the Dominican priory of San Esteban in Salamanca are much impressed by the sixteenth-century Escalera de Domingo de Soto, a stone stairway leading from the church garden to the cloister above it. Just as impressive is El Puente de Domingo de Soto, a plaza and a bridge that still connects the priory with Salamanca proper. Both were built in the late 1550s with proceeds from the sale of the many textbooks Soto had written. A philosopher and theologian of great renown, chaplain to Emperor Charles V and leading Dominican at the Council of Trent, Soto was as well known for his treatises *On Nature and Grace, On Right and Justice,* and *On the Cause of the Poor* as he was for his textbooks. Thus, were I to use Soto to celebrate Hispanic philosophy today, there is no dearth of materials from which I might draw. But since the focus here is on the Age of Discovery, I prefer to draw attention to a different facet of Soto's career, his role in a little known but very important discovery, that of the law of falling bodies.

Born at Segovia in 1494, Soto did his early studies in logic and natural philosophy under Thomas of Villanova at the University of Alcalá, earning the baccalaureate there in 1516. He then traveled north to the University of Paris to pursue higher studies in the College of Santa Barbara. There he studied under the Valencian, Juan de Celaya, under whose tutelage he became acquainted with the terminist physics then current in Paris. He completed the master's degree in arts and, while teaching in the arts faculty, began the study of theology. During this period he came under the influence of the Scottish nominalist John Major, who was then teaching at the College of Montaigu (along with two of Soto's fellow Segovians, Luis and Antonio Coronel), and the Spanish Thomist Francisco de Vitoria, who was lecturing at the Dominican priory of Saint-Jacques. In 1519, however, Soto's longing for Spain prompted his return to Alcalá, where he completed the course

in theology under Pedro Ciruelo; immediately thereafter he was given the chair of philosophy at the College of San Ildefonso. Much impressed by Vitoria and upset by internal conflicts at the college, he resigned that post in 1524 and decided to enter the Dominican Order. He became a novice at the priory of San Pablo in Burgos, where he was professed on 23 July 1525.[1]

The nature of Soto's contribution to early modern science can be appreciated by drawing a contrast between him and the Catalan humanist, Juan Luis Vives, who was born in Valencia two years before Soto, in 1492. Vives was also at the University of Paris in the early sixteenth century, along with a goodly number of teachers and students from the Iberian peninsula. Both he and Soto followed the same course of instruction in logic and in physics at the university. They studied the same subject matters, but they did so to very different ends.

On the one hand, we have the well-known story of Vives, the humanist who never returned to Spain and who was so disenchanted with the instruction he received at Paris that he spent much of his later life attacking the decadent Scholasticism being taught in Belgium and in England.[2] On the other hand, we have the lesser-known story of Soto, actually a countertheme to that of Vives. Soto returned to Spain and there, through his teaching first at Alcalá, then at Burgos and Salamanca, laid important foundations on which Galileo Galilei would one day build his science. For the one, Vives, Paris was a disaster; for the other, Soto, it was the starting point for innovations that would prompt the great French historian of science, Pierre Duhem, to hail him as the * "Scholastic precursor" of Galileo.[3] So we pose a question: what was the difference between Vives and Soto that made their lives diverge so markedly, such that historians would accentuate the anti-logic, anti-science stance of the one as contrasted to the pro-logic, pro-science stance of the other, and so evaluate the *fortuna* of Scholastic teaching at Paris in such radically different ways?

The question, so posed, is difficult to answer. But the key to the answer lies in the way logic was taught in the arts faculty at Paris, against which Vives directed many of his attacks. The basic text there was the *Summulae logicales* of Peter of Spain. This gave rise to the "summulist"

1. For a brief survey of Soto's life and works, see the entry written by W. A. Wallace in *Dictionary of Scientific Biography*, s.v., "Soto, Domingo de." The definitive account is Vicente Beltrán de Heredia, *Domingo de Soto, Estudio biográfico documentado* (Salamanca: Biblioteca de Teologos Españoles, 1960).

2. Juan Luis Vives, *In Pseudo-dialecticos*, ed. and trans. C. Fantazzi (Leiden: E. J. Brill, 1979).

3. Pierre Duhem, *Études sur Léonard de Vinci*, 3 vols. (Paris: Hermann et Fils, 1906–13), 3:263–583, esp. 555–62.

tradition, a series of textbooks used in the Parisian colleges to drill schoolboys in dialectical subtleties. These may have sharpened the memories and argumentative skills of youths, but they were ultimately to prove useless for serious work in philosophy and theology. A more humanistically inclined student such as Juan Luis Vives would easily have been repelled by the exercises to which he was subjected. The experience seems to have been so bad, in fact, that it diverted him from "the learning of the Schools" and caused him to focus instead on the *litterae humaniores* as the repository of true knowledge. So he turned to the new Renaissance movement, humanism, and devoted all of his energies to it.

Soto's reaction to the drilling associated with summulist teaching at Paris was quite different from that of Vives. Probably he realized that the *Summulae* were never intended to be the whole of logic; rather, they were simply a propaedeutic that would exercise students in abstract logical forms and so prepare them for the more difficult discipline of Aristotle's *Organon,* culminating in the material logic of the *Posterior Analytics.* The *Posterior Analytics* provided a methodology essential to scientific reasoning that would find its best exemplification in mathematics and in the science embodied in Aristotle's *Physics.* More persevering scholars like Soto could endure the summulist training and pass beyond it to do original work that would bear fruit in the Scientific Revolution of the seventeenth century. Two innovations were particularly important: the development of the demonstrative *regressus* found in the first book of the *Posterior Analytics,* and the search for new ratios of motions, including the *uniformiter difformis* relationship, that could be used to revise Aristotle's teaching on falling motion in the seventh book of the *Physics.* Both of these can be traced back to Soto, whose path in this respect differed markedly from that of Vives.

This essay, as its title indicates, is devoted to Soto's achievement and its sequel. But it is important to issue a *caveat* at the outset. The innovations to be discussed were not "writ large" in the intellectual history of the times and for that reason are not well known, even among historians of science. For the most part they do not appear in sixteenth-century texts but have to be dug out of manuscript sources. A surprising amount of this material is preserved in rare books and manuscripts produced at the time in Spain and Portugal. That explains the latter part of my title, "The Iberian Roots of Galileo's Science." And yet no great generalizations can be expected to emerge from this study. History, even the history of science, unfolds in capricious fashion and only in retrospect can any rationality be discerned in its unfolding.

SUMMULIST PROFESSORS AT PARIS

The easiest way to begin is to provide an overview of the summulist teachers at Paris in the early sixteenth century to show what their intellectual interests were and how these led them into more fruitful fields of scholarship than the teaching of schoolboy dialectics. Our focus will be on professors who went beyond formal logic to write commentaries on Aristotle's *Analytics*. These instructors we divide into two groups, the first including those whose later work was mainly in the field of mathematics, the second, those who produced commentaries on the physical works of Aristotle.

The summulists who studied the *Analytics* and whose main contributions were to mathematics were all Spaniards of distinction. They may be treated here in the order of their birth: Pedro Sanches Ciruelo (1470–1554), Juan Martínez Silíceo (1486–1557), and last but not least, Gaspar Lax (1487–1560). Ciruelo began his studies at Salamanca, then came to Paris in 1492 to complete his education. While pursuing courses in theology he supported himself by "the profession of the mathematical arts," as he later explained. While at Paris he published a treatise on practical arithmetic that went through many printings; this was accompanied by his revised and corrected editions of Thomas Bradwardine's treatises on speculative arithmetic and speculative geometry. About the same time Ciruelo produced an edition of the *Sphere* of Sacrobosco, along with the questions on it by Pierre d'Ailly; this was reprinted repeatedly and became the main source for instruction in astronomy at Paris. Returning to Spain in 1515 he taught at the University of Alcalá, where Soto was among his students and where he produced other textbooks, among which was a complete course in mathematics, both pure and applied, including optics and music.

Juan Martínez Silíceo began his studies in Valencia before going to Paris in 1507, where he studied physics under one of Vives's teachers, Jean Dullaert of Ghent. While at Paris he published an *Ars arithmetica* in 1513, then a treatise on the use of the astrolabe, and finally a complete course in logic, all before returning to Spain in 1517. Actually, he was called back to teach philosophy at Salamanca *secundum methodum parisiensem*, "following the Parisian method," which he did with distinction. Later he was named Archbishop of Toledo and raised to the rank of Cardinal in the Roman Church.

Finally there is Gaspar Lax, also Vives's teacher at Paris, who along with Dullaert, by Vives's own testimony, regretted the many years he had spent there teaching the trivialities of *Summulae* doctrine. Lax began his studies at Zaragoza, and returned there to teach mathematics

and philosophy after a long career at Paris. A prolific writer, while at Paris he turned out so many tomes on terminist logic he became known as "the Prince of Parisian *sophistae*." But he also had serious interests in mathematics, publishing an *Arithmetica speculativa* based on Boethius and a formalistic treatise on ratios or *proportiones* that went beyond the earlier works of Euclid, Jordanus de Nemore, and Campanus of Navarre. His views on the ratios of motions are not found in that treatise, but they are expounded in a later work published at Zaragoza, his *Quaestiones phisicales* of 1527, wherein he recapitulates in systematic form the teachings of Thomas Bradwardine, William Heytesbury, and Richard Swineshead at Oxford and their terminist counterparts in fourteenth-century Paris. What is important about that work is the many times Lax uses terms such as *suppositio* and *uniformiter difformis*, terms that assume importance in our later discussion.

Expositors of the *Summulae* who turned their attention to the physical sciences include a number of those already mentioned: John Major (1467–1550), Jean Dullaert (1470–1513), Juan de Celaya (1490–1558), and Domingo de Soto (1495–1560). To these we would add the name of a Belgian Dominican, Peter Crokaert of Brussels (1465?–1514). Crokaert is significant for his adding Thomism to the nominalist and Scotist influences at Paris deriving from John Major. First studying under Major and then teaching nominalism at Major's college, the College of Montaigu, in 1503 Crokaert entered the Dominican Order in the priory of Saint-Jacques. There he converted to Thomism, embracing it so enthusiastically one would have thought he was a Thomist all his life. At Saint-Jacques he taught Francisco de Vitoria and also influenced Soto, who was Vitoria's friend and frequented Saint-Jacques at the time. Crokaert wrote commentaries on all of Aristotle's *Organon* as well as on the *Physics* and the *De anima*, and is particularly important for redirecting his Parisian contemporaries to the thought of St. Thomas Aquinas.

Dullaert had also studied under Major at Paris and there in turn taught Celaya, Silíceo, and Vives. An Augustinian friar, Dullaert published* questions on the *Physics* and *De caelo* of Aristotle (1506, 1511, 1512), and was working on the *Analytics* and the *Meteorology* at his death in 1513; interestingly, Vives himself put the *Meteorology* into print in the following year, prefacing it with a brief biography of his teacher. Earlier Dullaert had revised and edited Jean Buridan's questions on the *Physics* and two of the physical works of another Augustinian, Paul of Venice. He also was preparing a general edition of the writings of St. Albert the Great, based on previously unedited manuscripts he himself had discovered, but unfortunately, this was never completed.

No less universal in his interests was Dullaert's disciple, Juan de Celaya. Born in Valencia around 1490, Celaya went to Paris in the early years of the sixteenth century, studied there under Lax and Dullaert, and completed the M.A. around 1509. As an arts master, Celaya numbered Soto among his students, as already mentioned. He also taught with the Portuguese mathematician Alvaro Thomaz and learned from him "calculatory" techniques. These techniques assume prominence in Celaya's questions on the *Physics* (1517), which not only summarize the teachings of the English Mertonians, the Paris terminists, and the Paduan *calculatores,* but also expose the teachings of Aristotle *secundum triplicem viam,* as he put it, "according to the threefold way of St. Thomas, the realists, and the nominalists." A prolific writer, Celaya also turned out commentaries on *De caelo* (1517), *De generatione* (1518), the *Posterior Analytics* (1521), and the *Ethics* (1523), all written in the same "threefold way." Following this last work, Celaya left Paris in 1524 and returned to Valencia, where he taught theology and was made rector *in perpetuo* of the university. In his later years he lost interest in nominalist teachings and devoted himself instead to the Aristotelian-Thomistic tradition.

Fortunate indeed was Soto in having had Celaya as his philosophy teacher at Paris. Soto's main teaching as a Dominican was at Salamanca, where he first taught in the *studium generale* at the priory of San Esteban. Then, during the academic year 1531–32, he substituted for Francisco de Vitoria, who held the "prime chair" of theology at the University of Salamanca. So successful was he that the next year he was elected to the "vesper chair" of theology, a post he held for sixteen years. It was at this time that he found his theology students to be poorly prepared in philosophy. Having already published a *Summulae* (1529), to this he now added a *Dialectica* that exposed all of the *Organon,* including the *Posterior Analytics* (1543), and a commentary and questionary on the *Physics* (1551), the last of which figures importantly in our account.

Note that all of the professors we have just surveyed may with justice be referred to as *summulistae,* for they all either taught or wrote expositions of the *Summulae* of Peter of Spain. Many of them did so to support themselves while continuing their studies at the University of Paris. Some, it is true, became so engrossed with formal logic that it dominated their interests for a considerable period. But note here a parallel in the present day, namely, the many philosophers, especially in England and the United States, who became so fascinated with mathematical logic, parts of which were adumbrated by the summulists, that they used it to spawn a new mode of philosophizing we now call "analytical philosophy." What is noteworthy about those we have discussed

is that they were all scholars with broad intellectual interests who made significant contributions to sixteenth-century thought, contributions possibly more significant than those of their twentieth-century counterparts.

THE ENIGMA OF DOMINGO DE SOTO

A good illustration of such a contribution is associated with an expression used by *calculatores* in the summulist tradition, namely, *uniformiter difformis*, which translates into English as "uniformly difform." When applied to the velocity of motion, the sense of the expression would be that the velocity is "difform," that is, varying or of different form, but that it is doing so "uniformly" or in an unvarying way. To use the modern idiom, to say that a motion is "uniformly difform" is to say that it is uniformly accelerated, that is, that its velocity is changing and so the motion is accelerating, but that the change is regular and so the acceleration is uniform. Now historians of science commonly teach that Galileo Galilei was the first to discover the laws of falling bodies, among which is the fact that their motion, in free fall, is uniformly accelerated, in other words, that their motion is *uniformiter difformis* with respect to time. One can imagine therefore the surprise that attended Pierre Duhem's pronouncement in 1913, reasserted by Marshall Clagett in 1959, that Galileo was not really the first to discover this, for it was already known to Domingo de Soto, who stated it some eighty years before Galileo in his questions on Aristotle's *Physics*, published at Salamanca in 1551.[4] How did Soto come to make this remarkable statement, and when he did, why did he present it only in matter-of-fact fashion, as though everyone already knew it? These questions pose what has been referred to by Alexandre Koyré as "the enigma of Domingo de Soto."[5]

Almost twenty-five years ago I published an essay on that enigma.[6] I was able to verify then that Soto truly makes such a statement. After defining motion that is uniformly difform with respect to time, he notes that this motion is

4. For Duhem, see the previous note; Marshall Clagett, *The Science of Mechanics in the Middle Ages* (Madison: The University of Wisconsin Press, 1959), 555–56.

5. In Koyré's essay on "The Exact Sciences" in the second volume of René Taton's four-volume *History of Science*, entitled *The Beginnings of Modern Science*, trans. A. J. Pomerans (New York: Basic Books, 1964), 94–95.

6. W. A. Wallace, "The Enigma of Domingo de Soto: *Uniformiter difformis* and Falling Bodies in Late Medieval Physics," *Isis* 59 (1968): 384–401; reprinted and enlarged in W. A. Wallace, *Prelude to Galileo: Essays on Medieval and Sixteenth-Century Sources of Galileo's Thought* (Dordrecht-Boston: D. Reidel Publishing Co., 1981), 91–109. *

properly found in objects that move naturally and in projectiles. . . . For when a heavy object falls through a homogeneous medium from a height, it moves with greater velocity at the end than at the beginning. The velocity of projectiles, on the other hand, is less at the end than at the beginning. And what is more, the first increases uniformly difformly, whereas the second decreases uniformly difformly.[7]

Soto goes on to explain that the falling body will cover the same distance during its fall as another body moving at half the velocity with uniform speed, which he calculates out to yield the correct distance of fall.

Here is not the place to review the details of my earlier essay. Suffice it to mention that Soto's use of the expression *uniformiter difformis* has to be set against the way it was used in the fourteenth century by the Oxford "calculators" of Bradwardine's school and Parisian terminists of Jean Buridan's school, in the fifteenth century by Paduans, and in the early sixteenth by his predecessors at Paris. Practically everyone prior to Soto who treated the expression did one of two things: either (1) they viewed it in a logical or imaginative way, without any reference to motions in the real world, or (2) they employed a "two-variable" schema when discussing it, with the result that they always tied uniform difformity with respect to time to uniform difformity with respect to space and never separated the two. Only two of Soto's predecessors, William Heytesbury and Juan de Celaya, used the one-variable schema, defining uniform difformity with respect to time alone, but neither of these thought to apply this definition to any motion in the real world. Domingo de Soto alone illustrated the definition with an example, and when so doing made the important statements cited above.

Why Soto supplied this particular example has puzzled scholars for * a long time. My initial reply was that Soto was intent on combining his early training in the nominalist tradition with his later commitment to Thomism, and that this prompted his exemplification of this nominalist expression in the real world. This would accord well with Soto's own admission that he was "born among the nominalists and reared among the realists." Apparently he was proud of this dual intellectual heritage, for, when questioned about nominalist elements in his questions on the *Physics,* he protested that he would not remove them lest in taking out the weeds he might tear up the wheat. In his biography of Soto, Vicente Beltrán de Heredia notes that both Soto and Vitoria felt that some nominalist teachings were superior to those of their realist confreres, particularly in physics and in ethics, and resented attempts to impose any orthodoxy on them in this regard.[8]

7. Quoted in ibid., 400 (reprint, 106).
8. Beltrán, *Domingo de Soto,* 22.

A more difficult problem is whether Soto had empirical evidence for* characterizing falling motion as uniformly accelerated. He provides none in his *Physics* questionary, and scholars have generally taken this to mean that he had none. But the question is far from settled. In this connection we should note that Soto had finished writing his commentary and questions on the *Physics* up to book VII, where the passages I have quoted are to be found, early in 1545, at which time he was called to the Council of Trent. To make them quickly available to students his texts were printed in an incomplete edition of 1545, which does not contain the passages about falling motion. But Soto returned from Trent in 1550 and then finished both texts, printed at Salamanca in 1551. While en route to or while present at Trent, in northern Italy, Soto could have become acquainted with experimental work being done there on laws of fall, and this would have buttressed his rejection of the traditional Aristotelian teaching.

Little is known about such experimental work, but what is known is suggestive. As early as 1544, it appears, tests were being performed to show that Aristotle was wrong in his claim that heavy bodies will fall to the ground at uniform speeds directly proportional to their weights. Benedetto Varchi, in his *Questioni sull'Alchimia* finished by that date, in a discussion of experimental evidence relating to the motion of heavy bodies, mentions the findings of Francesco Beato, a Dominican philosopher at Pisa, and Luca Ghini, a Bolognese physician and botanist, as contesting Aristotle's claim. Likewise, Giovanni Battista Bellasco of Brescia, in a work entitled *Il vero modo di scrivere in cifra* published in 1553, inquires why it is that a ball of iron and one of wood fall to the ground at the same time. In the same year Giovan Battista Benedetti published his *Resolutio omnium Euclidis problematum,* and a year later his *Demonstratio . . . contra Aristotelem,* in both of which he also attacks Aristotle's ratios of motions. Now Benedetti held in high regard another Spanish Dominican, Petrus Arches, who had told him that criticisms of Aristotle's dynamic laws were being discussed in Rome the summer before he prepared his *Demonstratio.* Evidence such as this implies that tests of Aristotle's laws of falling bodies were being performed in Italy in the early 1550s. The fact that Beato, Arches, and Soto were all Dominicans enhances the possibility that Soto learned of such experimentation during his travels through Italy to Trent. If so, it could have provided the background for the example he gave of uniformly accelerated motion upon his return to Salamanca.[9]

9. W. A. Wallace, "Science and Philosophy at the Collegio Romano in the Time of* Benedetti," in *Cultura, Scienze e Techniche nella Venezia del Cinquecento* (Venice: Istituto Ve-

SOTO'S INFLUENCE ON GALILEO

A second enigma associated with the famous passage in Soto's *Physics* is whether or not the passage was known to Galileo and so could have influenced his formulation of the principle of uniform acceleration in free fall. Now Soto's *Physics*, both commentary and questions, became quite popular and went through nine editions in the second half of the sixteenth century, the penultimate of which was published at Venice in 1582, just as the young Galileo was beginning his study of the *Physics* at the University of Pisa. So it is not impossible that Soto's text was known to Galileo and, if so, could have been the direct source from which the latter took the law of falling bodies. Supporting this conjecture is Galileo's having mentioned Soto's *Physics* in one of his early notebooks. Pierre Duhem, whom I have mentioned earlier, seized on Galileo's use of *uniformiter difformis* in that context to claim for Soto the title of "Scholastic precursor of Galileo."

Now Duhem's thesis, what historians of science call the "continuity thesis," has claimed my attention for two decades. Over that time I have come to the conclusion that Duhem's original claim is unfounded, but that there are still good grounds for believing there was an influence of Soto on Galileo. My argument is not based directly on the Galileo manuscript cited by Duhem, but rather on the sources that lie behind that manuscript. We now know that these were Jesuit teaching notes Galileo appropriated for his own use while teaching or preparing to teach at the University of Pisa. Through a study of books and manuscripts written by Jesuits in the century following the publication of Soto's *uniformiter difformis* doctrine, mainly in Italy, Spain, and Portugal, we can make the case for an influence on Galileo that was indirect and mediated by Jesuits, many of whom either came from or worked on the Iberian peninsula.[10]

This new argument, like Duhem's, uses the *uniformiter difformis* doctrine deriving from Soto, but adds another link in the person of Franciscus Toletus, Soto's favorite student at Salamanca. After himself teaching in the arts faculty at Salamanca, and already a priest, Toletus entered the newly formed Society of Jesus at Toledo in 1558. In 1559 he was sent to Rome, while still a novice, to teach at the Collegio Ro-

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neto di Scienze, Lettere ed Arti, 1987), 126; reprinted in W. A. Wallace, *Galileo, the Jesuits, and the Medieval Aristotle* (Hampshire: Variorum Publishing, 1991).

* 10. W. A. Wallace, "The Early Jesuits and the Heritage of Domingo de Soto," *History and Technology* 4 (1987): 301–20; reprinted in *Galileo, the Jesuits, and the Medieval Aristotle*, 1–20. See also W. A. Wallace, "Late Sixteenth-Century Portuguese Manuscripts Relating to Galileo's Early Notebooks," *Revista Portuguesa de Filosofia* 51 (1995): 677–98.

mano. There he wrote a series of philosophy textbooks that set the pattern for Jesuit teaching in philosophy over the next three decades. In brief, Toletus brought Soto's doctrine to Rome, and through his influence it was so widely propagated that it ultimately reached the ears of Galileo.

Precisely how this was done could easily be the subject for another* lecture. Here we can simply note that the expression *uniformiter difformis* occurs not only in Toletus's writings but also in books of two Spanish Jesuits who taught philosophy in Rome, Benedictus Pererius from Valencia and Francisco Suárez from Granada. All three recognize the significance of that expression, and Toletus in particular remarks that terms like this "should be carefully considered in order to understand many matters that are met with in physics."[11] Toletus's advice, it seems, is quite apparent in the writings of Antonius Menu, who taught in Rome from 1577 to 1582 and frequently quoted the *Doctores Parisienses* in his lectures. Like Toletus he was acquainted with the calculatory tradition developed at Merton College in Oxford and then applied to physical problems by the Parisians. Menu in turn was succeeded by three other Jesuits, Paulus Vallius, Mutius Vitelleschi, and Ludovicus Rugerius, who taught natural philosophy at the Collegio Romano between 1585 and 1592 and who, like Menu, left fairly complete records of their lectures. All mention the works of Burley and Bradwardine, of Heytesbury and Swineshead, in their notes. Vitelleschi, for example, cites experimental evidence against Aristotle's exposition of the ratios of motions in book VII of the *Physics* and refers his students to Bradwardine's *De proportione motuum* for a better view. Rugerius likewise discerns difficulties with Aristotle's ratios and sends his students to Soto and Toletus for more satisfactory accounts of how the velocity of fall varies over the distance of fall.[12]

These three professors in particular are of interest because we can detect parallels in their lecture notes for practically the entire content of Galileo's notebook referred to above, providing excellent evidence that Galileo appropriated the material in the notebook from lectures such as theirs. Three other professors at the Collegio might also be mentioned in this regard. One is Christopher Clavius, the mathematician who taught the *Sphere* of Sacrobosco there from 1564 to 1595 and some of whose work is clearly reproduced in the notebook. An-

11. In his *Commentaria una cum quaestionibus in octo libros Physicorum* (1573), bk. 4, q. 12, cited by Christopher Lewis, *The Merton Tradition and Kinematics in Late Sixteenth and Early Seventeenth Century Italy* (Padua: Editrice Antenore, 1980), 86.

12. W. A. Wallace, *Galileo and His Sources: The Heritage of the Collegio Romano in Galileo's Science* (Princeton: Princeton University Press, 1984), 184–91.

other is Stefano del Bufalo, who taught the *Physics* in 1596–97 and makes good use of the *Calculatores* and the *Parisienses* in his lectures. Yet another is Andreas Eudaemon, who taught the same course in the following year. Eudaemon is of interest for having left in manuscript a treatise on the motion of projectiles which starts out with the interesting *suppositio* that "every natural agent acts *uniformiter difformiter* on a quantified subject when applied to it," and then goes on to show the many ways in which the related expression, *uniformiter difformis*, applies to the agent's effects.[13]

There is thus clear evidence of Soto's influence, mediated by Toletus, on lectures given at the Collegio Romano in the latter part of the sixteenth century. Similar evidence is available to show an influence in the lecture notes of Jesuits in Portugal, at Evora and Coimbra, some of which derives from teaching materials sent them by the Collegio Romano. In that connection I have examined sets of notes composed in 1570, 1582, 1587, and 1588 in these two *studia* by the Jesuits Juan Gomez de Braga, Luis de Cerqueira, Antonio del Castelbranco, and Manuel á Lima, respectively. Another set of notes written anonymously in 1580 apologizes for not discussing at length the velocity of motion and simply states that such "questions are treated more fully by Domingo de Soto and can be studied there. For this reason, and especially because of limitations of time, we will pass over them quickly." Cerqueira, lecturing in 1582, apparently took this reference to Soto seriously. After defining what is meant by uniformly difform motion, he writes:

> Such a motion is said to be *uniformiter difformis* with respect to time, and it is found in heavy and light bodies when they move naturally, since the more they depart from their starting point the greater is the velocity with which they move.[14]

Manuel á Lima expands on this discussion in his lectures of 1588, writing:

> . . . if one and the same stone were to descend from the middle of a tower and later from its top, it would descend much more swiftly at the end of the later motion than at the end of the earlier. For the longer the space that is traversed the greater is the impetus impressed by levity and gravity throughout the motion, since it is continually intensified until the object arrives at its natural place.[15]

From these citations, and I could give more, there is little doubt that

13. Wallace, "The Early Jesuits," 311 (reprint, 11).
14. Ibid., 313 (reprint, 13).
15. Ibid., 314 (reprint, 14).

Soto's teaching on falling motion was preserved among the Jesuits both in Rome and on the Iberian peninsula during the period under discussion.

But how, you wonder, could teachings such as this have influenced Galileo in arriving at the laws of free fall? The answer to that is most interesting, but it can be sketched here only briefly. It is now known that at the time he wrote the notebook cited by Duhem, around 1590, Galileo did not yet possess the correct law governing natural acceleration. In fact, even in 1604, when he was beginning his experiments on free fall at Padua, he was speculating that the speed of fall varies uniformly difformly with distance of fall, not with time of fall, as we now know it to do. The two—velocity increase with respect to time and velocity increase with respect to distance—are not the same, and this was recognized by the *Calculatores,* as we have already seen. Shortly after 1604, we also know, Galileo became aware of his mistake. How did he learn of the error? One possibility is the Jesuits who were then teaching in Padua, for they had a *studium* there until 1606, at which time they were expelled from the Republic of Venice. Two of those Jesuits, by an odd coincidence, just happened to be Paulus Vallius and Andreas Eudaemon. We also know from Jesuit correspondence that Eudaemon personally discussed problems like this with Galileo himself. So either he or Vallius could easily have informed Galileo of the correct formulation, namely, that speed of fall increases uniformly with time of fall and not with distance traversed. Shortly after that it appears that Galileo performed experiments at Padua that discriminated between the two possibilities and so arrived at the correct law.[16]

SUPPOSITIONES AND THE *POSTERIOR ANALYTICS*

Earlier I mentioned Aristotle's *Posterior Analytics* and its key role in bringing to completion the logic course at the University of Paris. Now I would like to return to logic and make a few remarks relating to *suppositio,* a term much used by Peter of Spain and the summulists at Paris. When Toletus began to teach logic at the Collegio Romano in 1559, apparently he patterned his course on the logic contained in Soto's *Dialectica* of 1543. He also took care to preface it with a slim volume, entitled "Introductio in dialecticam," in which he gave in summary form the entire contents of the *Summulae.* (Soto himself had put out a second

16. Wallace, *Galileo and His Sources,* 269–80; see also W. A. Wallace, *Galileo's Logic of Discovery and Proof: The Background, Content, and Use of His Appropriated Treatises on Aristotle's* Posterior Analytics (Dordrecht-Boston: Kluwer Academic Publishers, 1992), 268–73.

edition of his *Summulae* in 1539, an indication that he did not class it with his *Juvenilia* but saw it still as helpful to students.) One of the topics treated in the *Summulae* is the supposition of terms, that is, how meanings are to be assigned to terms depending on the context in which they occur. Much of this material was tedious and onerous to students, and elicited complaints such as those we have seen voiced by Vives against Dullaert and Lax.

Apart from the supposition of terms, however, there is another use of *suppositio* that pertains to the principles on which demonstrations or scientific proofs are based. This is the supposition of propositions, which is not the same as the supposition of terms. In his scientific writings Galileo himself frequently says that he is demonstrating *ex suppositione,* that is, "from a supposition," but scholars have had great difficulty understanding precisely what he could mean by that expression. In my attempts to understand it, and I have been working on this now for some years, I came across another of Galileo's Latin notebooks, different from the one referred to by Duhem. This one, written like the other in Galileo's own hand, is still preserved in manuscript in the National Library in Florence, where it bears the simple title *Dialettica.* Oddly enough, though composed in 1588 or 1589, as I have found, it was never transcribed and so was left out of Galileo's *Opere* when they were published at the beginning of this century.

In 1988, four hundred years after it was written, William F. Edwards and I published the Latin text of that manuscript, along with a preliminary account of the sources on which it was based.[17] The notebook turns out to be a questionary on the *Posterior Analytics* of Aristotle, and it gives all the answers one would wish to understand the demonstrations proposed by Galileo in his scientific writings. In it we can find what it means to demonstrate *ex suppositione,* and, if we reflect on Galileo's scientific findings with his telescope and experiments on motion, why he used that expression to characterize and validate much of his later work.

What do you suppose is the source on which the logical questions in that notebook are based? I have already stated that the first notebook was taken from the writings of Clavius and possibly from those of three other Jesuits—Vallius, Rugerius, and Vitelleschi—all of whom cover materials very similar to those found in Galileo's manuscript. For the second notebook the case is much more clearcut. It was appropriated directly from the logic course taught at the Collegio Romano in 1587–

17. Galileo Galilei, *Tractatio de praecognitionibus et praecognitis* and *Tractatio de demonstratione,* ed. W. F. Edwards and W. A. Wallace (Padua: Editrice Antenore, 1988).

88 by Paulus Vallius, who later published a complete version of the course in two folio volumes of seven hundred pages each. If we study Galileo's compositon with the aid of those volumes we can find all we need to know about the scientific methodology and epistemology that underlies his life's work. Furthermore, on the basis of the evidence supporting the identification of Vallius as the source behind Galileo's second notebook, we now have very good reason to believe that Vallius was also the source behind his first.

One of the key teachings in Galileo's notebook on logic is that on the demonstrative *regressus*, the process whereby in the physical sciences one can discern the cause of a phenomenon from the effects it produces, and then regress, or go back in the opposite direction from cause to effect, and so provide scientific demonstrations of the phenomenon being investigated. Historians have associated this *regressus* with two well-known Paduan Aristotelians, Agostino Nifo and Jacopo* Zabarella. Additionally, a few remarks Galileo makes in his scientific writings have led some, such as Ernst Cassirer and John Hermann Randall, Jr., to claim that Galileo became acquainted with that methodology from his association with Aristotelian professors during his teaching days at Padua. Now we know that such could not be the case. The logical notebook was written at Pisa before Galileo moved to Padua, and it provided the methodology he was using long before he experimented with falling motion at Padua or made his discoveries with the telescope there, for which he quickly became famous. And its source was not the Paduans but the Jesuits, who first were taught the *regressus* by Franciscus Toletus in his *Logica* of 1572, basing his teaching on Soto's exposition of the *Posterior Analytics* given thirty years earlier at Salamanca.

This discovery is truly momentous, and it has led me to go back over all of Galileo's scientific writings to see the extent to which he used *suppositiones* and the demonstrative *regressus* in them. The complete results of that study will be found in my two volumes, *Galileo's Logical Treatises*, which provides an English translation of his logical questions along with commentary and notes, and *Galileo's Logic of Discovery and Proof*, which explains not only the context in which these questions must be located but also the way Galileo relied on their teaching throughout his scientific career.[18] The second volume, in particular, shows that despite Galileo's disagreements with the peripatetics over their teachings on motion and the heavens, he himself followed Aristotelian method-

18. W. A. Wallace, *Galileo's Logical Treatises. A Translation, with Notes and Commentary, of His Appropriated Latin Questions on Aristotle's* Posterior Analytics (Dordrecht-Boston: Kluwer Academic Publishers, 1992). See note 16 for the full citation for *Galileo's Logic of Discovery and Proof.*

ology when making his own discoveries. As he was to write to Fortunio Liceti, and this only sixteen months before his death, he may have disagreed with the Aristotelians in the universities on many matters, but in matters of logic he had been a peripatetic all his life![19]

Philosophers of science, to be sure, will find much to contest in this new thesis, for it implies revisionism of the most drastic sort. No longer can the "Father of Modern Science" be seen as breaking away from the learning of the schools and using radically new methods to make discoveries about nature and the universe. The methods he used were old, tried, and true. Especially in their combining mathematical with physical reasoning, they owe much to the fusion of nominalist and realist thought that began to appear almost a century earlier in the writings of Domingo de Soto.

What, then, can be said about Soto's influence on Galileo? Was the Pisan physicist actually influenced by the *uniformiter difformis* teaching I have been sketching out here? Can we see in Soto, as Pierre Duhem was tempted to see, Galileo's precursor in uncovering the law of falling bodies? Surely the case is not apodictic, the evidence far from overwhelming. And yet one would be quite rash to discount completely the materials I have presented. Particularly when we consider the notebook containing Galileo's physical questions in light of his newly available notebook containing the logical questions, we begin to discern elements of continuity that have been completely overlooked before that now require a complete reassessment of Galileo's intellectual heritage. Undoubtedly he owed a debt to the Jesuits from which he appropriated those notebooks. And they in turn owed a debt to those who preceded them, a debt, for the cases in which we have been interested, that extends all the way back to Domingo de Soto.

To summarize: the title of this essay is "Domingo de Soto and the Iberian Roots of Galileo's Science." The summulist tradition I have been sketching originated in England, was nourished in France, but it found its most enthusiastic proponents among the Spanish and Portuguese who studied at the University of Paris in the early decades of the sixteenth century. In a most unusual way it was transplanted to Rome by Spanish Jesuits, and there it took up new roots in the Society of Jesus. It also continued to flourish on the Iberian peninsula, especially among the Jesuits, although unfortunately it is not conspicuous

19. The letter was written on 14 September 1640. An English translation is given in W. A. Wallace, "Aristotle and Galileo: The Uses of ΥΠΟΘΕΣΙΣ *(Suppositio)* in Scientific Reasoning," in *Studies in Aristotle,* ed. D. J. O'Meara (Washington, D.C.: The Catholic University of America Press, 1981), 75; reprinted in Wallace, *Galileo, the Jesuits and the Medieval Aristotle.*

in the *Cursus philosophicus* that was edited by the Jesuit faculty at Coimbra, the famous *Conimbricenses*. One has thus to search in lecture notes, in manuscript sources, to discern its presence. But it was there nonetheless, especially among professors who taught the *Physics* and who later shared the same interests as Galileo. Once this is understood, considerably new light is cast on the twofold enigma of Domingo de Soto.

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II

The Enigma of Domingo de Soto: *Uniformiter difformis* and Falling Bodies in Late Medieval Physics *

THE AIM of this discussion is to cast light on what Alexandre Koyré has referred to as "the enigma of Domingo de Soto."[1] Soto was a Spanish Dominican who, in the early sixteenth century, studied at the University of Paris, returned to Spain, and at the University of Salamanca composed a commentary and questions on the *Physics* of Aristotle (*c.* 1545) along with an imposing series of works on political philosophy and theology.[2] In a much-quoted passage in his questions on the *Physics* Soto associates the concept of motion which is *uniformiter difformis*—an expression deriving from the English *Calculatores*—with falling bodies, and he indicates that the distance of fall can be calculated from the elapsed time by means of the so-called Mertonian mean-speed theorem.[3] The casual way in which Soto introduces this association has led some to speculate that this was generally known in his day and that he merely recorded what had become common teaching in the early sixteenth century. "But if this is the case," writes Koyré, "why was de Soto alone in putting [these views] down on paper? And why did no one else before

* The Catholic University of America, Washington, D.C. Work for this paper was supported by a grant (GS-862) from the National Science Foundation, whose help is gratefully acknowledged.

[1] In his essay on science in the Renaissance in René Taton (ed.), *History of Science*, Vol. II: *The Beginnings of Modern Science, from 1450 to 1800*, trans. A. J. Pomerans (New York: Basic Books, 1964), pp. 94–95.

[2] The best documentary study of Soto's life and works is Vicente Beltrán de Heredia, O.P., *Domingo de Soto: Estudio biográfico documentado* (Salamanca: Convento de San Esteban, 1960).

[3] As will be explained, the application of the expression *uniformiter difformis* to falling bodies is equivalent to stating that the motion of such bodies is uniformly accelerated. The method of calculating the distance traversed in such a motion was first worked out at Merton College, Oxford, by a group of scholars usually referred to as the Mertonians, or *Calculatores*. Their method consisted in replacing the uniformly varying speed by an average or mean value and then using this to compute the distance of travel; the equivalence on which this technique was based has come to be known as the "mean-speed theorem." Actually the method was applied in a broader context to all types of change, whether quantitative or qualitative, and the theorem could be referred to more generally as the "mean-degree theorem." Likewise the expression *uniformiter difformis* was applied to all kinds of change, but in what follows we shall restrict ourselves exclusively to applications to local motion and thus omit any discussion of the extensive literature that developed relative to other applications.

Galileo . . . adopt them?[4] These questions, neither of which is easily answered, *pose the enigma of Domingo de Soto.

The answer to the riddle should be forthcoming from a study of the teachings of Soto's predecessors and contemporaries, and it is such a study that provides the background for the present paper. The results show that Soto is still probably the first to associate the expression *uniformiter difformis* (with respect to time) with the motion of falling bodies. The association itself, however, is not completely fortuitous: it appears to be the result of a progression of schemata and exemplifications used in the teaching of physics from the fourteenth to the sixteenth centuries. I intend to trace this development and to show the extent to which Soto's presentation of the material was novel. Soto's uniqueness, it seems to me, consists in having introduced as an intuitive example the simplification that Galileo and his successors were later to formulate as the law of falling bodies. How Soto came to his result appears to be a good illustration of the devious route that scientific creativity can follow before it terminates in a new theoretical insight that is capable of experimental test.

For the sake of convenience I shall divide the presentation on the basis of its approximate chronology, treating successively those schemata and exemplifications used in discussing local motion in the fourteenth, fifteenth, and sixteenth centuries. The background is, of course, Aristotelian, but in the foreground are to be found the various Mertonian and nominalist distinctions that figured prominently in the emerging science of mechanics.

Fourteenth Century

In the first schema to be discussed the basic distinctions were foreshadowed in the works of Gerard of Brussels and of Thomas Bradwardine, but they came to be known by the mid-fourteenth century through the writings of Bradwardine's disciples William Heytesbury and Richard Swineshead.[5] These distinctions were applied generally to the intensification of changes or motions; as applied to local motion "intension" became synonymous with velocity or its change, and thus various qualifying adjectives such as "uniform" and "difform" came to have kinematical significance. A uniform motion U is one with constant velocity v, whereas a difform motion D has a changing velocity. Further, a motion may be uniform in either of two senses: with respect to the parts of the object moved, symbolized $U(x)$, in the sense that all parts of the object move with the same velocity; or with respect to time, symbolized $U(t)$, in the sense that the velocity of the object as a whole remains constant over a time interval. This distinction may also be applied to difform motions, yielding the two corresponding types, $D(x)$ and $D(t)$. With difformity, moreover, a further series of distinctions may be introduced. Motion that is difform with respect to the parts of the object moved may be either uniformly difform, $UD(x)$, in the sense that there is a uniform (spatial) variation in the velocity of the various parts of the object, or difformly difform, $DD(x)$, in the sense that there is no such uniform (spatial) variation. Again, motion may be uniformly difform with respect to time, $UD(t)$, or difformly difform in the same sense, $DD(t)$. Both of these in turn may be subdivided on the basis of the direction of the change—that is, whether it is increasing or decreasing—to yield uniformly accelerated motion, $UD_{acc}(t)$, and uniformly decelerated motion, $UD_{dec}(t)$, or alternatively,

[4] Koyré, in Taton, *op. cit.*, p. 95.

[5] For a general survey of this development see Marshall Clagett, *The Science of Mechanics in the Middle Ages* (Madison: Univ. Wisconsin Press, 1959).

difformly accelerated motion, $DD_{\text{acc}}(t)$, and difformly decelerated motion, $DD_{\text{dec}}(t)$. The resulting eight possibilities, all of which are capable of exemplification, are shown in Schema I; the symbols on the right will serve to number the particular types of motion in the schema and the examples that were proposed to concretize their definitions.

Schema I

- v
  - U
    - $U(x)$ I-1
    - $U(t)$ I-2
  - D
    - $D(x)$
      - $UD(x)$ I-3
      - $DD(x)$ I-4
    - $D(t)$
      - $UD(t)$
        - $UD_{\text{acc}}(t)$ I-5
        - $UD_{\text{dec}}(t)$ I-6
      - $DD(t)$
        - $DD_{\text{acc}}(t)$ I-7
        - $DD_{\text{dec}}(t)$ I-8

The complete articulation of all the subdivisions of Schema I was not given, to my knowledge, by any author before Soto, although the main lines of the division were already implicit in Heytesbury (1335). All of the English *Calculatores*, however—and in this designation I include Heytesbury as well as Swineshead (*c.* 1340), the English logician Robert Feribrigge (*c.* 1367), and the pseudo-Bradwardine (the author of the *Summulus de motu,* published in 1505)[6]—were content to define the various kinds of motion in abstract and mathematical terms, without illustrations from the physical universe. However, on the Continent, at the University of Prague, John of Holland (*c.* 1369)[7] repeated most of the divisions in Schema I and when explaining his definitions provided four examples: motion of type I-1 he illustrated with a falling stone (*motus lapis deorsum*), all of whose parts move at the same speed; type I-2, with an object (*mobile*) moving in uniform translation, or, alternatively, with a sphere (*spera*) in uniform rotation; type I-3, with the motion of the ninth sphere (*nona spera*), some of whose parts move more slowly than others even though the whole rotates uniformly; and type I-5, with the example of Socrates (*Sortes*) continually accelerating his walking speed. In passing, when defining types I-5 and I-6, John of Holland referred to the definitions given by the *Calculatores,* a reference that serves to align him with the Mertonian tradition. His work, to my knowledge, provides the fullest exemplification of Schema I of those written in the fourteenth century.

Writing at about the same time as John of Holland but at the University of Paris, Albert of Saxony[8] and (somewhat earlier) Nicole Oresme[9] utilize another

[6] On Robert Feribrigge see Clagett, *ibid.,* pp. 630-631; the text of the *Summulus de motu* is given by Clagett also, on pp. 445–462.

[7] For the relevant texts, *ibid.,* pp. 247–250.

[8] Albert of Saxony, *Questiones super quatuor libros Aristotelis de celo et mundo. . .* (Venice: 1520).

[9] See the excerpt from Oresme's *On the Configuration of Qualities* in Clagett, *Mechanics in the Middle Ages,* pp. 347–381. For the dating of Oresme's scientific writings see Clagett's article on Oresme in the forthcoming *Dictionary of Scientific Biography* (Scribner's); this has been pre-printed by the American Council of Learned Societies (New York, 1967) as a sample article for the guidance of authors preparing articles for the *Dictionary.*

way of classifying types of local motion. Both refer to motions being uniform or difform according to parts, $U(x)$ and $D(x)$, and according to time, $U(t)$ and $D(t)$, although Albert prefers to speak of the latter motions as "regular" and "irregular."[10] Again, both are concerned to supply examples and in so doing group the types of motion in a way that influenced later writers. Rather than consider one independent variable at a time, they take two variables together and speak of motion being, for example, "uniform and regular," which may be symbolized as $U(x) \cdot U(t)$, or "uniform and irregular," symbolized as $U(x) \cdot D(t)$, or "difform and regular," $D(x) \cdot U(t)$. The four possibilities that result from this classification are given in Schema II.

SCHEMA II

$$v \begin{cases} U(x) \cdot U(t) \dotfill \text{II-1} \\ U(x) \cdot D(t) \dotfill \text{II-2} \\ D(x) \cdot U(t) \dotfill \text{II-3} \\ D(x) \cdot D(t) \dotfill \text{II-4} \end{cases}$$

The examples provided by Albert and Oresme are particularly interesting in that a falling body is used as an illustration of local motion. Thus Albert's example of type II-2 is a heavy object (*grave*) or a falling stone (*lapis*): in the latter case the motion is uniform, $U(x)$, because all parts of the stone move with the same velocity at any instant, but irregular, $D(t)$, because the stone moves faster "at the end than at the beginning."[11] His example of type II-3, on the other hand, is a wheel (*rota*) whose motion is difform, $D(x)$, because "the parts close to the axle do not move as fast as those close to the circumference," but regular, $U(t)$, because the angular velocity of the whole remains constant.[12] A third example, type II-1, is described by Albert as follows:

> Similarly note a third possibility, that there is no difficulty in a motion being both uniform and regular at the same time: for when a heavy object descends in a medium

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[10] The Latin text reads as follows (*Questiones*, lib. 2, quest. 13):

> Regularitas autem motus attenditur ex parte temporis, ita quod motus ille dicitur regularis quando ipsum mobile movetur eque velociter in una parte temporis sicut in alia. . . . Verumtamen sciendum est quod aliqui distinguunt de uniformitate motus dicentes quod potest attendi vel ex parte partium mobilis, vel ex parte partium temporis. Uniformitas primo modo dicta est omnino eadem cum uniformitate distincta contra regularitatem; sed uniformitas secundo modo dicta est eadem cum regularitate. Sed illi non utuntur ita proprie uniformitate sicut nos utimur secundum dictas descriptiones.

[11] A fuller excerpt from the Latin text (*ibid.*):

> Ulterius sciendum est quod non est inconveniens aliquem motum esse uniformem et non esse regularem; patet de motu gravis deorsum in medio uniformi quod movetur uniformiter, quia una eius pars movetur ita velociter sicut alia, et tamen non movetur regulariter, quia movetur in fine velocius quam in principio.

Earlier in this question Albert gave the same example:

> . . . sicut si lapis aliquis descenderet, non obstante quod ille motus in fine esset velocior quam in principio; tamen diceretur uniformis secundum propriam significationem vocabuli ex eo quod una medietas illius lapidis descenderet ita velocior sicut alia.

[12] *Ibid.*:

> Similiter non est inconveniens aliquem motum esse regularem et tamen non esse uniformem; patet de rota que in equalibus partibus temporis equalia spatia describeret, talis enim motus rote esset regularis, non tamen uniformis, postquam [sic] partes illius rote centrales non moverentur ita velociter sicut partes circumferentiales.

whose resistance is so regulated that the heavy object covers equal distances in equal parts of time, the motion of the heavy object would be both uniform and regular.[13]

This example is peculiar in that it is more complicated than it need be: a stone in uniform translational motion would satisfy the case, as it did for John of Holland. Albert's example, however, is consistent with the discussions of the English *Calculatores* relating to motion through various resistive media and was perhaps suggested by them.

In the texts I have analyzed, Albert gives no example of type II-4—difform and irregular motion. Nicole Oresme, however, does exemplify all four types for Schema II, although he is cryptic in doing so. In place of the wheel for type II-3 he mentions the movement of the heavens (*celum*) as being difform and regular. Then he goes on: "Conversely, the movement downward of a heavy body can be uniform and irregular [II-2], and it can be also uniform and regular [II-1], or even difform and irregular [II-4]."[14] Here Oresme makes the falling body cover all three remaining possibilities, although he gives no indication as to what modalities must be superimposed on its motion in order to satisfy the various definitions.[15]

To provide background for an interpretation of authors to be considered later in this paper, it will be convenient to note the mathematical descriptions of falling motion that were proposed by Albert and Oresme. In their commentaries on Book II of Aristotle's *De caelo et mundo* both discuss a variety of possibilities but do not use the terms *uniformiter, difformiter,* or *uniformiter difformiter.* Oresme mentions that the velocity of fall either increases with time arithmetically toward infinity or else increases with time convergently (as do proportional parts) toward a fixed limit; he elects for the first possibility. Albert also mentions these two possibilities and elects similarly, although in his first mention he is ambiguous as to whether he regards the velocity as varying linearly with the time of fall or with the distance of fall. Later he is more explicit and mentions three additional possibilities: (1) the velocity receives equal increments in the proportional parts of time (thus going to infinity exponentially within a finite time); (2) the velocity receives equal

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[13] *Ibid.*: Similiter sciendum est tertio non esse inconveniens aliquem motum simul esse uniformem et regularem, sicut si aliquod grave descenderet in aliquo medio sic proportionato ex parte resistentiae, quod illud grave in equalibus partibus temporis equalia pertransiret spatia; tunc enim motus illius gravis esset uniformis et regularis simul.

[14] The Latin text is provided by Clagett, *Mechanics in the Middle Ages,* p. 375:

Motus vero gravis deorsum potest esse econverso uniformis et irregularis et etiam potest esse uniformis et regularis, vel difformis et irregularis. Sed non est possibile quod motus circularis sit uniformis. Verumptamen in sequendo modum loquendi consuetum vocabo quamcunque regularitatem nomine uniformitatis et irregularitatem nomine difformitatis. . . .

In his English translation of this, which differs from the one I provide, Clagett alters the punctuation and inserts an ellipsis that seems to me to change the sense. He translates: "Conversely, the movement downward of a heavy body can be 'uniform,' and 'irregular' or 'regular' " (p. 355). Prof. Clagett informs me that he has a new text with a correct translation in press, in a work titled "Nicole Oresme and the Medieval Geometry of Qualities and Motion."

[15] "Uniform and irregular" is obviously the case of a body falling freely in a uniform medium. "Uniform and regular" could be the case of a body falling in a medium whose resistance continually increases so as to prevent the possibility of a velocity increase (as in Albert of Saxony's example); whereas "difform and irregular" could be the case of a body falling freely in a uniform medium and rotating as it falls.

increments in the proportional parts of the distance traversed (thus going to infinity exponentially within a finite distance); (3) the velocity receives equal increments in equal parts of the space traversed (thus going to infinity linearly as a function of distance). Albert here elects the third possibility, showing that his earlier ambiguity should be resolved in favor of a spatial rather than a temporal variation. Clagett has analyzed all of these texts to show that none explicitly identifies falling motion as uniformly difform in such a way as to allow the Mertonian "mean-speed theorem" to be applied to it in unequivocal fashion.[16]

Schema II, it should be noted, did not enjoy the same popularity among later writers as did Schema I. However, types of motion that would fit into one or another of its categories were mentioned by the pseudo-Bradwardine, by Gaetano da Thiene, and by John Dullaert of Ghent. The principal importance of Schema II is that it introduced the two-variable concept and that it became the major vehicle for presenting falling bodies as exemplifications of the types of velocity variation in local motion being discussed in the late Middle Ages.

Fifteenth Century

Moving now to the fifteenth century, we come to a series of Italian writers associated with Paul of Venice (d. 1429), several of whose disciples wrote commentaries on the portions of Heytesbury's *Regule* concerned with local motion. These are preserved in a Venice edition of 1494,[17] which served to keep the "calculatory" tradition alive on the Continent long after it had ceased to be of interest in Great Britain. The most interesting of these treatises is the commentary of Gaetano da Thiene (d. 1465), who showed a knowledge of the terminology of Schemata I and II, but who proposed yet another alternative for classifying the various types of local motion.[18] This, like Schema I, influenced many authors and in fact dominated the tradition through the first half of the sixteenth century, up to the time of Soto's writing.

Gaetano differentiates uniform from difform motion both with respect to time and with respect to the parts of the object moved, as had earlier authors. He departs from them, however, in introducing a sixfold grouping that makes use of the two-variable concept of Albert and Oresme but allows for two more possibilities. For Gaetano a motion may be uniform either with respect to the parts of the object moved *alone*, or with respect to time *alone*, or with respect to the parts and to time *taken together*.[19] Similarly, a motion may be difform in the same three ways. The resulting six possibilities, written in an improvised notation (in which ~ stands for negation), are given in Schema III.

[16] *Mechanics in the Middle Ages*, pp. 553–555; see also Anneliese Maier, *An der Grenze von Scholastik und Naturwissenschaft*, 2. Auflage (Rome: Edizioni di Storia e Letteratura, 1952) pp. 214–215, 315–316.

[17] *Hentisberi de sensu composito et diviso* . . . (Venice: Bonetus Locatellus, 1494). This edition is described by Curtis Wilson, *William Heytesbury: Medieval Logic and the Rise of Mathematical Physics* (Madison: Univ. Wisconsin Press, 1960), p. 4, n., and *passim*.

[18] On Gaetano see P. Silvestro da Valsanzibio, O.F.M. Cap., *Vita e dottrina di Gaetano di Thiene, filosofo dello studio di Padova, 1387–1465* (2nd ed., Padua: Studio Filosofico dei Fratrum Minorum Cappuccini, 1949).

[19] *Expositio litteralis supra tractatum [Hentisberi] de tribus [predicamentis], De motu locali* (Venice: 1494), fol. 37[rb]:

> Unde notandum quod motus uniformis potest esse triplex: primo quo ad subiectum tantum; secundo quo ad tempus tantum et non quo ad subiectum; tertio quo ad subiectum et tempus simul.

SCHEMA III

$$v \left\{ \begin{array}{ll} U \left\{ \begin{array}{lll} U(x) & \cdot \sim U(t) & \ldots\ldots\ldots \text{III-1} \\ U(t) & \cdot \sim U(x) & \ldots\ldots\ldots \text{III-2} \\ U(x) & \cdot\, U(t) & \ldots\ldots\ldots \text{III-3} \end{array} \right. \\ \\ D \left\{ \begin{array}{lll} D(x) & \cdot \sim D(t) & \ldots\ldots\ldots \text{III-4} \\ D(t) & \cdot \sim D(x) & \ldots\ldots\ldots \text{III-5} \\ D(x) & \cdot\, D(t) & \ldots\ldots\ldots \text{III-6} \end{array} \right. \end{array} \right.$$

This schema is redundant: III-2 is equivalent to III-4, and III-1 is equivalent to III-5. Such redundancy, however, was quite common in medieval systems of division.

Having enumerated these possibilities, Gaetano gives an example of each one, thereby setting a precedent for most of those who were to adopt this particular method of classification. He illustrates type III-1 with the heavy object (*grave*) falling and type III-2 with the wheel (*rota*), as had Albert of Saxony for the analogous cases in his schema (II-2 and II-3); the same two illustrations but in the reverse order he attaches to types III-4 (*rota*) and III-5 (*grave*). His example for type III-3, on the other hand, is ambiguous. Gaetano speaks of a body descending "in a uniform space" (*mobile descendit in spacio uniformi*) and regards this as a motion that is uniform with respect both to time and to the parts of the falling object.[20] He makes no mention of the resistance increasing with the interval of fall, as does Albert of Saxony for the analogous case (II-1), and this seems in fact to be ruled out by his expression *in spacio uniformi*.[21] An alternative possibility could be an object being lowered at constant speed—not falling freely—but there is no positive suggestion of this in the text. Gaetano's example for the remaining case, type III-6, on the other hand is new—a wheel (*rota*) whose angular velocity is being continually increased (*movetur velocius et velocius*). Finally he mentions the example of a ball (*pila*) that falls and rotates as it does so: various components of its motion then illustrate the different types. Falling, it exemplifies type III-1; if rotating uniformly it exemplifies type III-2; if turning slower and slower, it exemplifies type III-6.[22]

There are other examples in Gaetano's commentary that are of interest. He mentions an object moving rectilinearly and supposes that it is neither contracting

[20] *Ibid.*:
Exemplum tertii, ut quando mobile descendit in spacio uniformi, et non velocitatur magis in una parte temporis quam in alia: tunc movetur equaliter et uniformiter quo ad magnitudinem, scilicet, quo ad omnia sua puncta, quorum tantum precise descendit unum sicut reliquum, et in equali parte temporis.

[21] The words *in spacio uniformi* are themselves puzzling, since 15th-century writers had no conception to match the modern notion of isotropic space. The words probably refer to a uniform *medium* and not to a uniform *space* understood in the strict sense.

[22] *Ibid.*:
Unde notandum quod quando pilla descendit rotando tunc ille motus multipliciter consideratur; nam inquantum descendit sic talis motus est uniformis quo ad subiectum et difformis quo ad tempus; quando vero pilla rotat, tunc aut rotat equevelociter aut tardius et tardius—si equevelociter ille motus est uniformis quo ad tempus et difformis quo ad subiectum; et postea considero illos duos motus, si secundo modo est difformis quo ad subiectum et quo ad tempus.

nor expanding as it moves, for the expansion or contraction would obviously cause a nonuniform motion of some of its parts. Along the same line he proposes the case of a wheel that rotates, but he now imagines the wheel to expand and contract during its rotation—a phenomenon that would explain further difformities in the motion of its parts. Another example is the placing of a cutting edge against a wheel to continually cut off the outermost surface, thus producing a difformity of the motion of the circumference. A more imaginative possibility is to have the inner parts of the wheel expand while the outermost surface is being cut off; this produces a more complicated variation in the difformity of the movement of the parts. Yet another example is a disc made of ice that is rotated in a hot oven: here the outermost surface continually disappears, and the velocity at the circumference becomes slower and slower; whereas the inner parts expand under the influence of the heat, and their linear velocity increases. A final example is that of a wheel that rotates and continually has material added to its circumference, as a potter might add clay to the piece he is working. Although the velocity of rotation is uniform, the linear velocity of a point on the circumference would increase, unless the entire wheel could be made to contract in the process, in which case it would remain constant.[23]

The foregoing examples can all be viewed as variations of the types of motion sketched in Schema III. Gaetano mentions also some of the types of motion that occur in Schema I; he gives definitions or kinematical descriptions of uniformly difform (I-5 and I-6) and difformly difform (I-7 and I-8) motions, but in these cases he follows the English *Calculatores* and gives no examples whatsoever—not even those already provided by John of Holland.

The remaining Italian commentaries on Heytesbury's *Regule* show more affinity with the latter part of Gaetano's commentary than with its earlier sections in which examples abound: the commentators restrict themselves, for the most part, to kinematical descriptions. Thus Messinus divides local motion into uniform and difform motion and gives a definition of uniform motion that applies only to uniformity with respect to time; he does stipulate, however, that the moving object must retain its quantitative dimensions throughout the motion, thereby implying that there be no change of the parts with respect to each other, and he further stipulates that the "space" passed over be neither contracting nor expanding during the motion.[24] When speaking of difform local motion he makes explicit the distinction between difformity with respect to time and difformity with respect to magnitude and says that an infinite number of possibilities exist for both types of difformity. The relation between distance and time can have any ratio one might wish, and the variation of velocities between respective parts of the moving object can be anything imaginable.[25]

[23] Gaetano, *Expositio litteralis*, fols. 37ra–40rb. These examples may seem overly fanciful to the modern reader, and yet without their help it becomes almost impossible to visualize the complex kinematical cases being discussed by Gaetano and his contemporaries.

[24] *Questio Messini de motu locali* (Venice: 1494), fol. 52va:

> Motus uniformis est per quem continue manentem equalem nec remissum nec intensum tantum de spacio pertransitur in uno tempore sicut in alio sibi equali, ceteris paribus, puta quod spacium non maneat condempsatum nec rarefactum, et mobile sit semper eiusdem quantitatis.

To speak of *spacium* as *condempsatum* or *rarefactum* is as puzzling as to refer to it as *uniforme* (see n. 21 above).

[25] *Ibid.*, fol. 52va:

> Motus difformis in infinitum variari potest: tam respectu temporis quam respectu magnitudinis; potest enim plus de spacio pertransiri in uno tempore quam in alio, scilicet, equali; et hoc in quacumque proportione volueris. Similiter in diversis temporibus inequalibus in quacumque proportione volue-

Angelo da Fossombrone,[26] on the other hand, reflects some of the concern for exemplification that is found in Gaetano da Thiene. Angelo's characterization of local motion, for example, stresses its priority in the order of nature and its essential division into upward and downward, the distinctions of uniform and difform being considered accidental. Like Messinus he is concerned with eliminating physical factors that would cause the moving body to expand or contract or would change the dimensions of the space through which it moves, and he wishes to define the types of local motion so as to rule out such possibilities.

Angelo follows Gaetano's Schema III, moreover, but supplies only three examples: for type III-3 he gives a moving object (*mobile*) that does not change its parts through condensation or rarefaction but continues in rectilinear motion with unchanging velocity; for type III-2 he cites a wheel (*rota*) that revolves and is moved by a constant force (*potentia*) exerted upon it; and for type III-1 in place of the customary falling body he provides the more abstract example of an object (*mobile*) all of whose parts move with the same velocity as the whole while this velocity is changing with respect to time.[27]

Bernardo Torni of Florence,[28] alone of this group, reverts to Schema II and mentions three of its four possibilities—the identical ones discussed by Albert of Saxony. He also mentions a few cases that would fit into Schema I. However, he gives no examples—intentionally, since he believes the reader knows enough to furnish his own (*casus tuipse scis formare*).[29]

From this survey of fifteenth-century Italy we can see that the development

ris potest spacium pertransiri equale; et in infinitum variari potest ex parte temporis; similiter ex parte magnitudinis eo quod una pars potest moveri velocius alia certa data. Unde talis motus est difformis: quo ad partes subiecti. Et hoc est motum variari respectu magnitudinis; et illud infinitis modis potest esse, sicut satis patet: quia qualitercumque velis imaginari quod una pars, alia velocius moveatur.

[26] *Supra tractatu*[*m*] *de motu locali* (Venice: 1494), fols. 64^{ra}–73^{rb}.

[27] *Ibid.*, fol. 64^{ra}:
Motuum autem localium tam uniformium quam difformium aliquis est talis, scilicet, uniformis vel difformis quo ad tempus; aliquis quo ad partes subiecti mobilis. Et aliquis potest esse motus uniformis quo ad tempus et etiam quo ad partes subiecti: ut si unum mobile moveatur non variatum intrinsecus per condensationem aut rarefactionem semper eodem gradu motus non intenso nec remisso motu simpliciter recto movetur uniformiter quo ad tempus, ut apparet per dictam descriptionem. Movetur etiam uniformiter quo ad partem subiecti moti, quia omnes partes omnino velocitate consimili moventur. Moveri enim uniformiter quo ad partes subiecti est omnes partes eque velociter moveri. Aliquid enim contingit uniformiter moveri quo ad tempus et difformiter quo ad partes subiecti, sicut rota mota circulariter ab eadem potentia continue equali nixu ita quod circumferentia ipsius continue equaliter movetur. Tunc patet quod totum movetur difformiter quo ad partes subiecti, quia velocius moventur partes versus circumferentiam quam versus centrum. Aliquid tamen movetur uniformiter quo ad partes subiecti et difformiter quo ad tempus sicut si tali motu simpliciter recto aliquod mobile moveatur omnino semper equali velocitate quo ad omnes suas partes qua movetur totum cum hoc tamen in partibus temporis intendendo vel remittendo motum suum, etc.

[28] *In capitulum de motu locali Hentisberi quedam annotata* (Venice: 1494), fols. 73^{va}–77^{va}.

[29] *Ibid.*, fols. 74^{rb}–74^{va}:
Scias tamen quod communiter ponitur ista distinctio. Aliquid uniformiter moveri intelligitur dupliciter: quo ad tempus, et quo ad partes subiecti; et eodem modo dupliciter contingit difformiter moveri. In superibus locuti sumus quo ad tempus. Verum id uniformiter movetur quo ad partes quando quelibet eque velociter cum alia movetur; difformiter vero quando una velocius et alia tardius. Ex quo patet quod stat *A* difformiter moveri quo ad tempus et uniformiter quo ad partes subiecti, et e contra. Et stat quo ad utrumque uniformiter moveri; et casus tuipse scis formare.
The last phrase suggests that examples were either discussed in class or left to the students as exercises.

there was somewhat ambivalent. As evidenced in the work of Gaetano, there is a concern for exemplification, with insistence on the case of the falling body, but this appears in the setting of Schema III with its two-variable classification similar to that used at the University of Paris, where *uniformiter difformis* with respect to time does not appear. As evidenced in the remaining commentators on Heytesbury, on the other hand, there are occasional references to types of motion that fit into Schemata I and II (including *uniformiter difformis*), but for these there is only kinematical description, no exemplification.

Sixteenth-Century Paris

A pronounced revival of interest in physical problems took place at Paris in the early part of the sixteenth century under the influence of the Scottish nominalist Jean Mair, whose disciples wrote a considerable number of "questionaries" on the *Physics* of Aristotle.[30] In these it was customary to incorporate treatments of local motion that borrowed heavily from such writers as Heytesbury, and thus there was once again a fusion of Mertonian and Parisian thought. Again, in this period a considerable number of scholars from the Iberian peninsula were studying at Paris, and as a result there was a diffusion of the new developments into Spain and Portugal within a few decades of their discussion at Paris. The various schemata we have been discussing figure in this new movement, and there is a growth of exemplification that prepares for the association of uniformly difform motion with the case of falling bodies.

* The first writer to prepare for this association was the Augustinian John Dullaert of Ghent, who edited the works of Paul of Venice, another Augustinian, and also wrote questions on the *Physics*.[31] Dullaert was a disciple of Jean Mair, and he seems to have brought about a blending of Parisian nominalist interests deriving from Albert of Saxony with the realist concerns that characterized the school of Paul of Venice.

Dullaert's exposition follows Schema III and gives illustrations for all six possibilities.[32] For type III-1 there is the usual example of the heavy body (*grave*) falling through a uniform medium; this case illustrates type III-5 also. For types III-2 and III-4 Dullaert uses Oresme's example of the motions of the heavenly sphere (*sphera celestis*) rather than the customary wheel. The illustration of type III-3 is Albert of Saxony's: a body falling through space whose resistance is so proportioned that it has uniform velocity with respect both to time and to all the parts of the subject. Type III-6, finally, is exemplified by the wheel (*rota*) that accelerates its rotation—identical with the case provided by Gaetano da Thiene.[33]

[30] On Jean Mair (Johannes Maior) and his school see Hubert Élie, "Quelques Maitres de l'université de Paris vers l'an 1500," *Archives d'histoire doctrinale et littéraire du Moyen Age*, 1950–1951, *18*:193–243; also R. G. Villoslada, S.J., *La Universidad de Paris durante los estudios de Francisco de Vitoria, O.P. (1507–1522)* (Rome: Gregorian Univ. Press, 1938), pp. 127–278.

[31] *Questiones super octo libros phisicorum Aristotelis necnon super libros de celo et mundo* (Lyons: 1512); Élie cites two previous editions, Paris 1506 and Paris 1511.

[32] *Ibid., Phisica*, lib. 3, quest. 1, fol. 65^{va}:
Quarto notandum est quod duplex est motus localis: quidam est uniformis et quidam difformis. Et triplex est motus uniformis: quidam est uniformis quo ad subiectum tantum, quidam quo ad tempus, quidam quo ad tempus et subiectum simul. . . . Similiter triplex est motus difformis, scilicet, quo ad subiectum, quo ad tempus, et quo ad utrumque.

[33] *Ibid.*:
Motus alicuius gravis cuius motus est velocior in fine quam in principio est uniformis sed non regularis, et motus alicuius sphere celestis est regularis et nullo modo uniformis, sed sto cum communi modo loquendi nec secum volo contendere de nomine.

Dullaert, however, does not stop here; he mentions some of the categories of Schema I and significantly furnishes a few examples also. For type I-3 he gives the motion of a heavenly sphere (*sphera celestis*) the parts of which move with uniformly increasing velocity as one goes from the pole to the equator. For type I-5 he cites the case of Socrates (*Sortes*) uniformily increasing his walking speed from zero to eight degrees—the example of John of Holland—and for type I-6 he mentions the converse case of Socrates decelerating his motion uniformly from a given speed to zero. He then explains that types I-7 and I-8 would be defined "in the opposite manner" (*opposito modo*), without giving examples.[34]

Apart from these divisions Dullaert mentions twice the velocity of descent of a falling body. In the first instance he gives it as an illustration of Albert of Saxony's terminology, as being a uniform but not a regular motion. The second mention comes when Dullaert refers to his exemplification of falling motion as being faster at the end than at the beginning, saying that some wonder how this can occur, since it would seem that the same ratio of force over resistance is maintained and thus the motion should be uniform. Dullaert postpones discussion of this case but says that the motion is actually faster at the end than at the beginning because of the accidental impetus that is built up in the fall.[35] He does not state that the motion will be uniformly difform, but is content to illustrate uniformly difform motion in terms of Socrates' walking speed.

Writing shortly after Dullaert, the Portuguese Alvaro Thomaz prepared a lengthy treatise *De triplici motu* patterned on the work of Swineshead but also incorporating materials from Gaetano da Thiene and others.[36] Like the English Mertonians, Alvaro is more concerned with kinematical descriptions of various types of motions than he is with examples drawn from the physical universe. He follows the initial classifications of Schema I, dividing motion into uniform and difform, and difform in turn into uniformly difform and difformly difform. Without defining these he immediately subdivides uniformly difform into a threefold classification similar to that used by Gaetano da Thiene: with respect to the subject *alone*, which may be symbolized $UD(x) \cdot \sim UD(t)$; with respect to time *alone*, symbolized $UD(t) \cdot \sim UD(x)$; and with respect to both subject and time *together*, symbolized $UD(x) \cdot UD(t)$.[37] He then gives the same threefold classification for

[34] *Ibid.*:
Et iterum duplex est motus difformis: quidam uniformiter difformis, quidam difformiter difformis. Et aliquis est motus uniformiter difformis quo ad subiectum, et est quandocumque partis subiecti dimidium tantum exceditur in velocitate ab extremo velocius moto quantum excedit aliud extremum, ut motus sphere celestis. De motu uniformiter difformi quo ad tempus ex illo facile patet quid sit dicendum, ut si Sortes deberet intendere motum suum per unam totam horam a non gradu usque ad 8, tunc quacumque parte illius motus accepta medium tantum excedit extremum quantum exceditur ab altero extremo. Et eodem modo si Sortes remitteret motum suum uniformiter ab aliquo certo gradu usque ad non gradum. Et opposito modo debet diffiniri motus difformiter difformis.

[35] *Ibid.*, fol. 65^{vb}:
Sed quia tangitur ibi de corpore gravi quod velocius descendit in fine quam in principio, dubitaret aliquis, et merito, unde hoc perveniat. Et supponamus quod si aliquod corpus grave ut 8 quid debeat moveri deorsum per aliquod medium subduple resistentie, tunc ex quo manebit semper eadem proportio activitatis super resistentiam sequitur quod non velocius descendit in fine quam in principio.—Sed ad hoc dicitur quod semper manebit eadem proportio essentialis sed non accidentalis, videlicet propter impetum. . . . Alias de hoc futurus est sermo.

[36] *Liber de triplici motu proportionibus annexis . . . philosophicas Suiseth calculationes ex parte declarans* (Paris: 1509).

[37] *Ibid.*, pars 3, tract. 2, cap. 1 (no foliation):
Motus uniformiter difformis (ut communiter definitur) est triplex: quidam est

difformly difform motion and mentions that it can also be applied to uniform motion. The implied schema, a variant of Schema III, is sufficiently different from this to be included in our study as something new, which we shall designate as Schema IV.

SCHEMA IV

$$v \begin{cases} U & \begin{cases} U(x)\cdot \sim U(t) & \ldots\ldots\ldots\ldots\ \text{IV-1} \\ U(t)\cdot \sim U(x) & \ldots\ldots\ldots\ldots\ \text{IV-2} \\ U(x)\cdot U(t) & \ldots\ldots\ldots\ldots\ \text{IV-3} \end{cases} \\ D & \begin{cases} UD & \begin{cases} UD(x)\cdot \sim UD(t) & \ldots\ldots\ \text{IV-4} \\ UD(t)\cdot \sim UD(x) & \ldots\ldots\ \text{IV-5} \\ UD(x)\cdot UD(t) & \ldots\ldots\ \text{IV-6} \end{cases} \\ DD & \begin{cases} DD(x)\cdot \sim DD(t) & \ldots\ldots\ \text{IV-7} \\ DD(t)\cdot \sim DD(x) & \ldots\ldots\ \text{IV-8} \\ DD(x)\cdot DD(t) & \ldots\ldots\ \text{IV-9} \end{cases} \end{cases} \end{cases}$$

Of the nine possibilities contained here, Alvaro discusses only three in detail, types IV-4 through IV-6, and he gives only one example, for type IV-4, the motion of the potter's wheel (*rota figuli*), although he does give a kinematical description of type IV-5 (which approximates I-5), an object (*aliquod mobile*) that moves from zero to a given velocity, uniformly increasing its speed.[38]

Another student of Jean Mair at Paris was the Spaniard Luis Coronel, a townsman of Domingo de Soto (both were from Segovia), who was possibly teaching at Paris when Soto came there as a young student. In his *Physice perscrutationes*[39] Coronel discusses many topics that were commonly dealt with in "questionaries" on the *Physics,* but he is sparing in his treatment of the types of local motion. He mentions, in passing, the division of local motion into rectilinear and curvilinear, and in discussing how the relative motions of two objects are to be compared, he states that either a uniformly difform or a difformly difform velocity would have to be reduced to an average value before a comparison could be effected. He gives no definitions of these types of motions, however, and provides no examples. Rather he refers the reader to the treatises of Heytesbury and Swineshead, with which he seems generally to agree, and otherwise does not think it worthwhile to waste his time over such matters.[40]

A similar treatment is to be found in the *Physica* of Juan de Celaya, another Spaniard who taught at Paris and who definitely numbered among his students Domingo (then Francisco) de Soto.[41] Celaya is not only acquainted with the

uniformiter difformis quo ad subiectum tantum, quidam quo ad tempus tantum, quidam vero quo ad subiectum et tempus simul.

[38] *Ibid.*:

. . . quo ad subiectum . . . Exemplum ut motus rote figuli . . . quo ad tempus . . . Exemplum ut si aliquod mobile incipiat moveri a non gradu continuo intendendo uniformiter motum suum per aliquod tempus; tunc talis motus est uniformiter difformis quo ad tempus.

[39] *Physice perscrutationes* (Paris: 1511); for a summary of Coronel's teaching see Pierre Duhem, *Études sur Léonard de Vinci,* Vol. III (Paris: Hermann, 1913), pp. 552–555.

[40] Coronel, *Physice,* lib. 3, pars 3, fol. 86^{rb}.

[41] See Beltrán de Heredia, *Domingo de Soto,* p. 16.

writings of Heytesbury and Swineshead, but makes explicit mention of the Italian commentators on Heytesbury. What is of particular interest in Celaya's exposition, however, is his departure from the two-variable type of schema (i.e., Schemata II, III, and IV) that dominated these treatments from Albert of Saxony to Alvaro Thomaz, and his return to the one-variable classification of Schema I. Although, like Coronel, he gives no examples, Celaya defines six of the eight types of motion in Schema I.[42] Possibly he was the writer who influenced Soto to adopt this classificatory schema in preference to the others, for he is the first among Soto's immediate predecessors, to my knowledge, to make use of it; besides, as already noted, he did teach Soto. Yet strangely enough neither he nor any other writer at Paris in his time seems to have thought to associate motion that is uniformly difform with respect to time with the case of falling bodies.

Sixteenth-Century Spain

The Spaniards we have discussed thus far studied at Paris and wrote their treatises while there. Other Spaniards under Parisian influence wrote questions on the *Physics* of Aristotle at universities in Spain: principal among these are Diego Diest,[43] whose *Questiones phisicales* appeared at the University of Saragossa in 1511; Diego de Astudillo,[44] who wrote at Valladolid in 1532; and Domingo de Soto, whose physical works were first published at Salamanco *circa* 1545. Since nominalist treatises seem to have received a less enthusiastic reception in Spain than they had in Paris, these writers, all of whom undertook to incorporate "calculatory" concepts into their courses on Aristotle, were careful to show that such concepts relate in some way to the physical universe. Partially for this reason and partially for pedagogical reasons they utilized considerably more exemplification than did those who wrote in Paris. This, coupled with the diversity of schemata that now seemed to require exemplification, set the stage for a new look at the old examples, for the introduction of some new ones, and for the eventual association of motion that is *uniformiter difformis* with the case of falling bodies.

Diest treats of uniform and difform motions at length, first in the context of Schema III, and then in a less systematic way that mentions elements of Schemata I and IV while discussing uniformly difform motion. He treats all six possibilities in Schema III, giving an example of each: for types III-1 and III-5 he cites the motion of a heavy object (*grave*) downward;[45] for types III-2 and III-4 he gives

[42] *Expositio . . . in octo libros phisicorum Aristotelis, cum questionibus . . . secundum triplicem viam beati Thome, realium, et nominalium* (Paris: 1517), fols. 81^{rb}–83^{vb}. Although giving no examples, Celaya does refer the reader to his treatment of uniformly difform qualities for a further understanding of these definitions: "Et qualiter iste diffinitiones habent intelligi ex declaratione qualitatis uniformiter difformis inferius apparebit" (fol. 81va).

[43] Diest, a native of Bolea in Spain, studied at Paris in the latter part of the 15th century. The full title of his work is *Questiones phisicales super Aristotelis textum, sigillatim omnes materias tangentes in quibus difficultates que in theologia et aliis scientiis ex phisica pendent discusse suis locis inseruntur* (Saragossa: 1511). For some further details on Diest, see Villoslada, *Universidad de Paris*, pp. 401–402.

[44] Astudillo taught at the College of Saint Gregory in Valladolid, which was staffed by Dominicans; here he composed his *Questiones super octo libros phisicorum et super duos libros de generatione Aristotelis* (Valladolid: 1532).

[45] Diest, *Questiones*, lib. 3, quest. 3, fol. 16va:

> Motus uniformis quoad subiectum tantum est motus uniformis quo aliquod mobile et quaelibet eius pars uniformiter movetur et difformiter quoad tempus, ut motus gravis deorsum totum et quaelibet eius pars uniformiter movetur sed difformiter quoad tempus quia velocius movetur in fine quam in principio.

the example of the wheel (*rota*);[46] for type III-3 he mentions a heavy ball (*sperula gravis*) falling downward in a medium that continually offers more and more resistance so that the velocity remains uniform;[47] and for type III-6 he suggests a wheel (*rota*) rotating with varying angular velocity.[48]

Having given and exemplified Gaetano's division of difform motion, Diest returns to this subject and provides an alternative division of difform motion into uniformly difform and difformly difform. He then embarks on a discussion of *uniformiter difformis* that is extremely interesting, for in it he states that the *uniformiter* part of this expression may be understood in various ways, meaning by this uniform variation either in a linear sense or in a logarithmic sense, which is clearly an innovation when compared to previous applications of this expression to the velocity of falling motion.

Diest first explains "uniformly" in the linear sense, as it had been commonly understood by his predecessors. He then proceeds to a second way of defining uniformly difform motion as follows:

> [This type of] uniformly difform motion occurs when a change in intensification, velocity, or quantity corresponds immediately to an extensive change in proportionable parts; briefly, when there is the same excess of the first proportionable part over the second as the second over the third, and so on.[49]

This statement is cryptic, and is explained in what follows immediately:

> This appears in local motion: it is commonly taught that a heavy object falling downward increases its speed uniformly difformly, so that it moves with a greater velocity in the second proportionable part than in the first, and with greater velocity in the third than in the second, and so on.[50]

The expression "proportionable part" is used interchangeably by Diest for "proportional part," and it means the same as geometric (or, in modern terminology, logarithmic) part. Thus Diest is saying that the velocity increase is the same in the first half of the body's fall as it is in the next quarter, as in the next eighth, and so on. In other words, the velocity of a falling object increases geometrically with the

[46] *Ibid.*:
Motus uniformis quoad tempus tantum est motus quo aliquod mobile et qualibet eius pars non uniformiter movetur, in equalibus tamen partibus temporis equalia spacia pertransit tale mobile, ut in rota mota notum est quod non quaelibet pars rote cum qualibet alia uniformiter movetur quia quanto aliqua pars magis accedit ad centrum tanto minus movetur . . . et tamen uniformiter movetur quo ad tempus, quia rota tantum spacium pertransit in una parte temporis quantum in alia sibi equali.

[47] *Ibid.*:
Motus dicitur uniformis quoad utrumque quando totum et quelibet pars uniformiter movetur et in equalibus partibus temporis equalia spacia pertranseunt, sicut est in linea mota uniformiter motu recto: potest exemplificari in sperula gravi mota deorsum supposito quod in medio continue maiorem et maiorem resistentiam inveniat taliter quod propter illam uniformiter moveatur in principio et in fine quo ad tempus.

[48] *Ibid.*:
Patet exemplum in rota mota magis in uno tempore quam in alio sibi equali.

[49] *Ibid.*:
Alio modo diffinitur sic: motus uniformiter difformis est quando in tali partes proportionabiles immediate secundum extensionem sunt immediate secundum intensionem vel velocitatem vel quantitatem; breviter quod qualis est excessus prime partis proportionabilis ad secundam talis est secunde ad tertiam, et sic procedendo.

[50] *Ibid.*:
Patet in motu locali: dicitur communiter quod grave descendendo doersum movetur uniformiter difformiter taliter intendendo motum quod velocius movetur in secunda parte proportionabili quam in prima, et in tertia magis quam in secunda, et sic procedendo.

distance of fall, going to infinity over a finite range. This, it must be noted, Diest proposes as common teaching (*dicitur communiter*), a statement that offers difficulty when one considers that Albert of Saxony had already considered this possibility only to reject it and that there seems to have been little or no discussion of it by the intervening authors. A way out of the difficulty would be to read into Diest's statement the understanding that the velocity increases by proportional parts, corresponding to the proportional parts of the distance traversed, which would be equivalent to holding that it increases linearly with distance of fall. This seems to have been "common teaching" from Albert of Saxony onward, and may have been what Diest intended, although the textual exegesis does not favor this interpretation.

This example, again, might appear to be an illustration of motion that is of type I-5, that is, uniformly difform with respect to time; actually it is not, for the independent variable in Diest's presentation is spatial (*partes secundum extensionem*) and not temporal—and here he foreshadows a difficulty that was to plague Galileo (in his early writings) and others in their attempts to formulate a correct law of falling bodies.[51]

Diego de Astudillo, like Soto, was a Dominican, and he was a close friend of one of Soto's first Dominican professors, the eminent jurist Francisco de Vitoria.[52] Astudillo's questions on the *Physics* (1532) cite Diest and most of the authors we have already mentioned, although his treatment is briefer than Diest's and not of as great significance. He works for the most part within Gaetano's schema, defining and exemplifying all six of its types. For type III-1 he provides the example of "all natural movements, for a stone [*lapis*] falling downward moves with equal velocity in all its parts, although with respect to time the velocity is greater toward the end than at the beginning, as is obvious from experience."[53] The reference to "experience" is significant, although it clearly cannot be taken in any metrical sense. The same example, as was usual, he associates with type III-5. For types III-2 and III-4 he prefers the illustration of the heavens (*celum*), as did Dullaert, giving as his (erroneous) reason that "the parts closer to the poles traverse more space than do those that are more remote."[54] For type III-3 he gives Albert of Saxony's example, observing that "this motion only seems capable of occurring per accidens, by reason of a resistance variation; e.g., if a stone [*lapis*] falls downward and encounters increasing resistance in the same proportion as its velocity of descent would be naturally increased."[55] The latter statement might be taken to imply that

[51] One of the pioneer discussions of this topic is Alexandre Koyré, *Études galiléennes*, Fasc. II: *La loi de la chute de corps—Descartes et Galilée* (Paris: Hermann, 1939).

[52] Both Astudillo and Vitoria taught at Saint Gregory's in Valladolid. Of Astudillo, Vitoria graciously remarked that he knew far more than himself but was not as good at marketing his ideas: "Fray Diego de Astudillo más sabe que yo, pero no vende tan bien sus cosas." See Villoslada, *Universidad de Paris*, pp. 304–305.

[53] *Op. cit.*, lib. 6, quest. 4, fol. 117rb:
Motus uniformis quoad subiectum est cuius omnes partes subiecti equa velocitate moventur ex parte subiecti, licet non ex parte temporis: sed maiori ex parte temporis et minori. Exemplum patet in omnibus motibus naturalibus. Lapis enim descendens deorsum, quantum ad omnes partes eque velociter movetur, tamen ex parte temporis, maior est velocitas circa finem quam principium, ut ex experientia patet.

[54] *Ibid.*:
. . . ut celum: quelibet enim pars celi tantum spacium describit in una hora quantum in alia, tamen non omnes partes equalia describunt spacia. Maiora enim spacia describunt partes pole propinquiores quam remotiores [sic].

[55] *Ibid.*:
Iste motus solum videtur inveniri posse per accidens ratione variationis alicuius resistentiae. Ut si lapis descendens deorsum, secundum proportionem secundum quam descendens naturaliter augeretur in veloci-

the stone's natural fall is uniformly accelerated, but at best this is only an inference; there is no clear indication, moreover, how "uniformly" should then be taken, particularly considering Diest's difficulties with the alternative meanings of this term. Finally, for type III-6 Astudillo introduces a new example, that of a "violent circular motion [*motus circularis violentus*] resulting from a projecting,"[56] evidently meaning by this some type of impelled rotation that comes gradually to rest, in which case there would be a velocity variation with respect both to parts and to time.

Astudillo, like Diest, gives the further twofold division of difform motion into uniformly difform and difformly difform and divides the first into two types, "with respect to the subject" and "with respect to time," thus touching on the various members of Schema I, types I-3 through I-8. He illustrates type I-3 with the heavenly body (*corpus celeste*), but merely defines the other types. After his definition of type I-5, that is, uniformly difform with respect to time, he adds cryptically, "as is apparent from the above."[57] This could mean that Astudillo thought he had already discussed this case in terms of an example or that the definition was so clear in light of the foregoing examples that it needed no illustration. Which meaning one takes depends on how he evaluates Astudillo's examples of the falling stone already discussed. I favor the latter alternative.

This brings us finally to Domingo de Soto, who had read Astudillo and most of the other authors already discussed, although he generally refrained from mentioning them by name. What is most remarkable about Soto is that he breaks completely with his immediate predecessors in rejecting all the two-variable schemata (II, III, and IV) and returns instead to Heytesbury's one-variable schema (I), which had been used by Juan de Celaya alone of all of the sixteenth-century writers. Soto gives a full explanation of this schema and then, in the fashion that had by then become customary, supplies examples for all its types. It is in this setting that he finally associates falling bodies with *uniformiter difformis* motion, taking *uniformiter difformis* in the precise sense of motion uniformly accelerated in time, to which he can, and does, apply the Mertonian "mean-speed theorem."

Soto gives the complete division of Schema I, plus definitions of all its types. Here we can only enumerate his examples, most of which had already been used by one or more of his predecessors.[58] For type I-1 he gives the case of a foot-length of stone being drawn over a plane surface (*si . . . pedalem lapidem trahas super planitiem*), while for type I-2 he mentions the invariant motion of the heavens (*in regulatissimo motu celorum perspectum est*), and for type I-3, the rotation of a millstone (*mola frumentaria*). Type I-4 offers more difficulty: Soto is unable to supply an example that involves local motion and so gives one that involves changes of quality (alteration), and this for the case of heating. His example is "a four-foot long object that is so altered in one hour that its first foot uniformly takes on a degree of heat of one, and its second uniformly the degree of two, and its third the

tate inveniant [sic] augmentum resistentie. Tunc enim utroque modo esset motus uniformis.

[56] *Ibid.*, fol. 117va:
. . . sicut patet in motu circulari violento, qui est per proiectionem.

[57] *Ibid.*:
Et patet ex dictis.

* [58] The following citations are from Soto's *Super octo libros physicorum questiones* (Salamanca: 1555), lib. 7, quest. 3; the earliest complete edition of this work was published at Salamanca in 1551 although an earlier printing, lacking parts of Bk. 7 and all of Bk. 8, appeared there *c.* 1545. There are no known manuscripts of the text. I plan to publish the principal portions of the Latin text with all significant variants together with an English translation and a commentary.

degree of three, etc."—for which he supplies the diagram of a step-function. He goes on to observe that "the present treatise is not at all concerned with this kind of alterative motion" but that he has given this example for the simple reason that a local motion of this type is hardly possible: rectilinear motion must be uniform in this respect, and rotary motion can only be uniformly difform—by its nature it cannot be difformly difform.[59]

Types I-5 and I-6 Soto defines in conjunction with each other and then observes that they are "properly found in objects that move naturally and in projectiles." He goes on:

> For when a heavy object falls through a homogeneous medium from a height, it moves with greater velocity at the end than at the beginning. The velocity of projectiles, on the other hand, is less at the end than at the beginning. And what is more, the [motion of the] first increases uniformly difformly, whereas the [motion of the] second decreases uniformly difformly.[60]

Then later on in the text while discussing the same case, "uniformly difform motion with respect to time," he removes any possible ambiguity as to his meaning by proposing the difficulty "whether the velocity of an object that is moved uniformly difformly is to be judged from its maximum speed, as when a heavy object falls in one hour with a velocity increase from 0 to 8, should it be said to move with a velocity of 8?"[61] His answer to this is clearly in terms of the Mertonian "mean-speed theorem," for he decides in favor of the average velocity (*gradus medius*) as opposed to the maximum. He justifies this with the illustration: "For example, if the moving object *A* keeps increasing its velocity from 0 to 8, it covers just as much space as [another object] *B* moving with a uniform velocity of 4 in the same [period of] time."[62] Thus there can be no doubt about his understanding of *uniformiter difformis* and how this is to be applied to the space traversed by a freely falling object.

To exemplify types I-7 and I-8, finally, Soto resorts to the motion of animals and to other biological changes, stating:

> An example would be if something were to move for an hour, and for some part [of the hour] were to move uniformly with a velocity of one, and for another [part] with a velocity of two, or three, etc., as is experienced in the progressive motion of animals. This kind of motion frequently occurs in the alteration of animals' bodies, and perhaps it can take place in the motion of augmentation and diminution.[63]

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[59] Soto, *ed. cit.*, fol. 92[vb]:
Et licet de hac specie motus alterationis nihil ad presens negotium, subiecimus tamen hoc exemplum idcirco, quod motus localis haud quaquam esse potest difformiter difformis quo ad subiectum. Quoniam rectus quidem nequit ullo modo difformis: omnes enim partes continue equaliter moventur; circularis vero, omnis est uniformiter difformis.
Obviously Soto is thinking only of the rotation of a rigid, nondeformable body and not the more imaginative cases mentioned by Gaetano da Thiene (see above).

[60] *Ibid.*:
Hec motus species proprie accidit naturaliter motis et proiectis. Ubi enim moles ab alto cadit per medium uniforme, velocius movetur in fine quam in principio. Proiectorum vero motus remissior est in fine quam in principio; atque adeo primus uniformiter difformiter intenditur, secundus vero uniformiter difformiter remittitur.

[61] *Ibid.*, fol. 93[vb]:
Utrum velocitas mobilis uniformiter difformiter moti sit denominanda a gradu velocissimo, ut si grave decidat in una hora velocitate a non gradu usque ad 8, dicendus sit moveri ut 8?

[62] *Ibid.*, fol. 94[ra]:
Exempli gratia, si *A* mobile una hora moveatur intendendo semper motum a non gradu usque ad 8 tantumdem spatii transmittet quantum *B*, quod per simile spatium eodem tempore uniformiter moveretur ut 4.

[63] *Ibid.*, fol. 92[vb]:
. . . ut si ita res aliqua moveretur per horam, ut per aliquam partem uniformiter

The illustrations apply mostly to type I-7, but his mention of diminution at the end (although not strictly a local motion) indicates that he is also aware of decreasing variations and thus implicitly includes type I-8 in his exemplification.

The further details of Soto's analysis of falling motion, together with its influence *on later thinkers, must await treatment elsewhere. The materials presented here, however, should help clear up at least part of "the enigma of Domingo de Soto." The contribution of the Spanish Dominican was not epoch-making, but it was significant nonetheless. Of the nineteen authors considered in this paper he alone thought of systematically providing examples for the simplest of the four schemata used—that which considers only one independent variable at a time. The others who were interested in exemplification—and these were mostly late-fifteenth-century or sixteenth-century writers—worked in the context of two-variable schemata, and this generally precluded the possibility of their even considering the case of motions that are uniformly difform with respect to time.[64] All of Soto's examples, of course, like those of his predecessors, were proposed as intuitive, without empirical proof *of any kind. Moreover, he and Diest, of all those considered, were the most venturesome in attempting to assign a precise quantitative modality to falling motion. Of the two, Soto was without doubt the better simplifier; he seems also to have been the better teacher, and he was philosophically more interested in unifying the abstract formulations of the nominalists with the physical concerns of the realists of his day.[65] Again, he had the advantage of time and of being able to consider more proposals. The strange alchemy of the mind that produces scientific discoveries requires such materials on which to work. It goes without saying that Soto could not know all that was implied in the simplification he had the fortune to make. But then, neither could Galileo, in his more refined simplification, as the subsequent development of the science of mechanics has so abundantly proved.

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moveretur ut 1, et per aliam ut 2 vel 3, etc. Ut est experiri in motibus progressivis animalium. Que quidem species motus crebro accidit in alteratione corporum animalium, et potest forsan contingere in motu augmenti et decrementi.

[64] The exception is Alvaro Thomaz, who did mention motion that is uniformly difform with respect to time in the context of his two-variable schema. His division, however, was so complex as to discourage any attempts at simple exemplification with natural examples.

[65] For a further discussion of this last point see my paper "The Concept of Motion in the Sixteenth Century," *Proceedings of the American Catholic Philosophical Association* (Washington, D.C.: Catholic University of America, 1967) pp. 184–195.

III

DOMINGO DE SOTO'S "LAWS" OF MOTION: TEXT AND CONTEXT

Almost thirty years ago I first documented Domingo de Soto's use of the expression *uniformiter difformis* (with respect to time) and its application to falling motion—a passage in which he anticipated Galileo's concept of uniform acceleration in free fall, and this by over eighty years.[1] I planned then, as I put it, "to publish the principal portions of his Latin text with all significant variants together with an English translation and a commentary."[2] Circumstances beyond my control have prevented me from carrying out that intention in full, although I have worked at the enterprise from time to time over the intervening years.[3] The occasion of honoring John Murdoch now gives me what may be my last opportunity to fulfill this long-outstanding promissory note.

SOTO'S WORK IN CONTEXT

To review the salient facts: Soto explains Aristotle's rules for the comparison of motions (which is what I intend by the expression "'laws' of mo-

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[1] William A. Wallace, "The Enigma of Domingo de Soto: *Uniformiter difformis* and Falling Bodies in Late Medieval Physics," *Isis* 59 (1968): 384–401. A preliminary version of that essay was presented in the Colloquium Series of the Department of the History of Science, Harvard University, on 11 May 1967. *

[2] Ibid., p. 399 n. 58.

[3] For details, see William A. Wallace, "The Concept of Motion in the Sixteenth Century,"* *Proceedings of the American Catholic Philosophical Association* 41 (1967): 184–95; "The 'Calculatores' in Early Sixteenth-Century Physics," *British Journal for the History of Science* 4 (1969): 221–32; "Mechanics from Bradwardine to Galileo," *Journal of the History of Ideas* 32 (1971): 15–28; "Experimental Science and Mechanics in the Middle Ages," *Dictionary of the History of Ideas*, 2: 196–205; "Galileo Galilei and the *Doctores Parisienses*," in *New Perspectives on Galileo*, ed. Robert E. Butts and Joseph C. Pitt (Dordrecht: Reidel, 1978), pp. 87–138; "The Early Jesuits and the Heritage of Domingo de Soto," *History and Technology* 4 (1987): 301–20; "Science and Philosophy at the Collegio Romano in the Time of Benedetti," in *Cultura, Scienze e Techniche nella Venezia del Cinquecento*, Atti del Convegno Internazionale di Studio "G. B. Benedetti e il suo tempo" (Venice: Istituto Veneto di Scienze, Lettere ed Arti, 1987), pp. 113–226; and "Late Sixteenth-Century Portuguese Manuscripts Relating to Galileo's Early Notebooks," *Revista Portuguesa de Filosofia* 51 (1995): 677–98. The first five of these essays, plus "The Enigma of Domingo de Soto," are reprinted in William A. Wallace, *Prelude to Galileo: Essays on Medieval and Sixteenth-Century Sources of Galileo's Thought*, Boston Studies in the Philosophy of Science, 62 (Dordrecht and Boston: Reidel, 1981); the next two, both published in 1987, in idem, *Galileo, the Jesuits and the Medieval Aristotle*, Collected Studies Series 346 (Aldershot, UK: Variorum, 1991).

tion" in my title), in his commentary on the *Physics*, Book 7, texts 35–37, first published at Salamanca in 1551–52.[4]

He then offers a critique and emendation of these "laws" in his questions on the *Physics*—particularly in his third and fourth questions on Book 7, which he precedes by a "Digression on Ratios" wherein he explains the meaning of *uniformiter difformis* and related expressions along with the mathematics of ratios and proportions required for their understanding. The questions were published in a companion volume to the commentary, at the same place and time.[5] An earlier edition of both commentary and "questionary" had appeared at Salamanca around 1545, but this was incomplete and did not extend to this portion of Aristotle's text. Seven subsequent editions of both commentary and questions were published down to 1613, the next five at Salamanca in the years 1555–56,[6] 1563, 1568–69, 1572, and 1582, the penultimate at Venice also in 1582, and the last at Douay in 1613. The late Vincente Beltrán de Heredia, who had worked for years in the Salamancan archives and prepared the definitive biography of Soto,[7] assured me that no manuscript copies of Soto's works on the *Physics* are extant. Thus the best one can hope for is a "variorum" edition of any relevant texts, showing the variants that might appear in the successive editions.

The burden of this essay is to present such an edition of the main portion of Soto's fourth question on the seventh book, which bears the title "Whether the velocity of motion depends with respect to its cause on the ratio of the ratios of the agents to their respective resistances?"[8] The third question dealt with the velocity of motion with respect to its effect, and it was in this question that Soto proposed his *uniformiter difformis* doctrine and explained how this could be used to calculate the distance of a body's fall through the application of the mean-speed theorem deriving from the English calculators. Since no significant variants were detected in the texts of this question cited in the "Enigma of Domingo de Soto" article, consid-

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[4] The full title is *Reverendi Patris Dominici Soto Sogobiensis theologi ordinis Praedicatorum in inclyta Salamanticensi academia professoris ac Caesareae Maiestati à sacris confessionibus super octo libros Physicorum Aristotelis. Commentaria.* This is from the second edition (Salamanca 1555–56), prepared after Soto had returned from the Council of Trent, where he served as the representative of the Dominican Order at the Council and as imperial theologian there of Charles V, who had also named him his personal confessor.

[5] This title reads simply *Reverendi Patris Dominici Soto Segobiensis Theologi ordinis praedicatorum super octo libros Physicorum Aristotelis. Quaestiones.* The publication of the two volumes at the same place and time is true of all nine editions, which were produced as companion volumes of a set.

[6] An edition of 1557–58 also appeared, but this is the same as the 1555–56 edition with only a change in the title page.

[7] Vincente Beltrán de Heredia, *Domingo de Soto, O.P., Estudio biográfico documentado* (Salamanca: Biblioteca de Teólogos Españoles, 1960).

[8] "Utrum velocitas motus attendatur ex parte causae penes proportionem proportionum, quae sunt agentium ad suas ipsarum resistentias."

eration of the question is postponed to later in the essay and priority is given to the fourth question. The latter contains Soto's teaching on the use of "ratio of ratios" (*proportio proportionum*) in laws of motion as first explained by Thomas Bradwardine. There are difficulties in this text, both in the way Soto presents it and in the way he was understood by later thinkers. Thus a study of the text and its context may prove helpful for gauging the value of Soto's work in its own right, as well as the influence it may have exerted down to the time of Galileo.

VARIANTS IN THE EDITIONS

In what follows, the various editions are designated by the first nine capital letters of the Roman alphabet, as follows:

A. Salamanca: Juan de Junta, 1545? (incomplete)
B. Salamanca: A. de Portonaris, 1551–52
C. Salamanca: A. de Portonaris, 1555–56
D. Salamanca: A. de Portonaris, 1563
E. Salamanca: Domingo de Portonaris, 1569
F. Salamanca: Domingo de Portonaris, 1572
G. Salamanca: Ildefonso de Terranova y Neila, 1582
H. Venice: Apud Franciscum Zilettum, 1582
J. Douay: Ex typographia Petri Borromans, 1613

Edition *A* ends with the first page of the first question on the seventh book, containing only 16 of the 42 paragraphs that make up that question in later editions, with the last words being "et remissio est proprie motus alterationis." *B* is the first complete edition, with commentary and questions on all eight books. *C*, the last to appear in Soto's lifetime (he died at Salamanca on 15 November 1560), is presented as the second edition, recently revised and corrected by the author.[9] All of the editions up to and including *F* are set in Gothic type; the remainder are in Roman. *H* is dedicated by the publisher to a Dominican friar, "Dominico Bolano, Iacobi filio, Patricio Veneto." The Flemish Dominican Jacobus de Brouwer prepared the last edition, *J*, and, as will be seen in the critical apparatus, in the process he made many emendations in the text.

Throughout the nine printings there are many changes of paragraphing, punctuation, and spelling. The first step in preparing a "variorum" edition of the questions on the seventh book was to standardize the paragraphing, using *C* as the basic guide. This yielded the following composition: Q. 1, 42

[9] The Latin reads: "Secunda aeditio nuperrimè ab Authore recognita, multisque in locis aucta et à mendis quàm maximè fieri potuit repurgata."

par.; Q. 2, 39 par.; Digression on Ratios, 62 par.; Q. 3, 53 par.; and Q. 4, 41 par., for a grand total of 237 paragraphs. Since the middle "Digression" presented the most difficult typesetting problem, with an intractable number of variants, this was eliminated at the outset, bringing the total for consideration down to 175 paragraphs. Soundings were then made in these paragraphs to identify significant word variants that could be traced through the various editions.[10] These yielded 45 terms or phrases to be checked throughout all four questions relating to the seventh book.

The first use to which this information could be put would be tracing the degree of dependence of the various editions on those that preceded them. Since any changes after *C* were made by the publisher (except for *J*, whose editor is known), the presumption would be that the publisher simply revised the previous edition in preparing his text. With a few exceptions, this generally seems to have been borne out. The first complete edition, *B*, obviously was based on *A*. The incomplete edition, *A*, contained only a small portion of Book 7, but this included 6 of our 45 variants, and all of these read the same in *B* and *A*. Here and in what follows, if the same readings are found in a subsequent edition,—in this case, *B*'s dependence on *A*,—this is written *BA* = *n*, where *n* is the number of variants, here 6. Only in comparisons with *A* is 6 the total possible, and thus the agreement in this case is 6/6. In all other comparisons all 45 variants have been evaluated, and thus 45 is understood to be the denominator in estimating the degree of agreement between any two editions.

The second edition, *C*, being a revised edition, would not be expected to read with either *B* or *A*, and this turns out to be the case, for *CB* = 27 (that is, 27 agreements out of 45 possible, or 27/45) and *CA* = 2 (that is, 2 agreements out of 6 possible, or 2/6). Edition *D* was probably based on *C*, but it reads equally with *C* and *B*, since *DC* = 22 and *DB* = 22. *E*, on the other hand, seems clearly to be based on *D*, since *ED* = 28, whereas *EB* = 17 and *EC* = 9. *F* likewise seems to have been based on its predecessor, since *FE* = 33, as compared to *FD* = 27, *FB* = 16, and *FC* = 15.

The first edition not from the Portonaris family and the first in Roman type, *G*, presents more of a problem. Its agreements read as follows: *GD* = 28, *GE* = 25, *GB* = 25, *GC* = 22, and *GF* = 21; these figures indicate that *D* probably served as its basic exemplar, but others such as *B* and *E* could have been consulted. The Venice edition, *H*, the first not from Salamanca, reads basically with *C*, its probable exemplar, since *HC* = 40, whereas *HD* = 29, *HB* = 26, *HF* = 14, and *HE* = 13. Finally, the Douay edition, *J*, is a maverick that does not correlate well with any previous edition; its agree-

[10] In this task I was assisted by Paul G. Zomberg, who as a graduate student had worked with me on the editorial staff of the *New Catholic Encyclopedia* and subsequently earned a doctorate in English at the University of Toronto.

ments are $JD = 28$, $JG = 25$, $JF = 24$, $JC = 24$, $JB = 23$, $JH = 22$, and $JE = 17$, and it probably reflects the editor's originality more than his dependence on any particular previous edition.

SOTO'S COMMENTARY ON ARISTOTLE'S RULES

Aristotle's seven rules for the comparison of local motions are given in chapter 5 of Book 7 of the *Physics* (249b27–250b7).[11] Soto refers to these rules in his fourth question, having provided his commentary on them in what he identifies as texts 35 through 38 of that book, following Averroes's division. There is little that is remarkable in Soto's commentary, since the rules he finds are basically those found by modern commentators, such as W. D. Ross (cited in note 11). One noteworthy feature, however, is found in his explanation of Aristotle's fourth rule in text 35, where Soto uses capital letters to designate the numerical values to be applied to motive force, resistance, space, and time. In edition *C* such letters are signaled by periods on either side of the letter (e.g., .A., .B., etc.). In Soto's *Quaestiones* on the *Physics* a similar technique is used, except that lowercase letters are there used in edition *C* to designate variables (e.g., .a., .b., etc.), and also numerals (e.g., .2., .4., .8., etc.) where the latter are required. Printers apparently had difficulty preserving this convention in their punctuation, and this led to confusion, as will be seen below.

In what follows, relevant excerpts are provided from Soto's commentary as found in edition *C*. The Latin text is given first and then its translation into English. In the Latin text, Soto's convention of putting periods on both sides of the capital letters designating variables has been followed, but not in the translation. Nor has this been done in the last sentence of the translation, where Soto does not give numerals but simply writes out the numbers (*unum*, *duo*, *quatuor*) and does not himself employ numerals. So as to clarify the sense, throughout the translation notes have been inserted that put Soto's version of Aristotles "laws" in modern notation, as is common practise among those working on late medieval science.

Latin text:

> [Text 35] ... Duae ergo hinc colliguntur regulae. Prima per divisione mobilis et multiplicationem spatii, potentia et tempore manentibus aequalibus. Si aliqua potentia movet aliquam resistentiam per aliquod spatium in aliquo tempore, eadem, vel aequalis potentia in eodem vel aequali tempore movebit

[11] For the Greek text and a commentary identifying and explaining the seven rules, see W. D. Ross, *Aristotle's Physics: A Revised Text with Introduction and Commentary* (Oxford: Clarendon Press, 1936), pp. 683–87.

subduplam resistentiam per duplum spatium. Ratio est quod qualis est proportio medietatis resistentia [*sic*] ad totam, talis est spatii ad spatium duplum.

Secunda regula est per divisionem mobilis et temporis, potentia et spatio invariatis. Si aliqua potentia movet per aliquod spatium aliquam resistentiam, eadem vel aequalis potentia per aequale spatium movebit subduplam resistentiam in subduplo tempore. Probatur: quia quanta est proportio totius resistentiae ad medietatem, tanta est temporis ad tempus....

Tertia ergo est per divisionem spatii et temporis, potentia et mobili invariatis. Si aliqua potentia moveat aliquam resistentiam per aliquod spatium in aliquo tempore, eadem vel aequalis potentia movebit eandem resistentiam per dimidium spatium in dimidio tempore....

Quarta regula est econverso per divisionem potentiae et resistentiae, spatio et tempore invariatis. Si aliqua potentia movet aliquam resistentiam per aliquod spatium in aliquo tempore, subdupla potentia movebit subduplam resistentiam per idem spatium in eodem tempore. Ut si .A. potentia moveat .B. resistentiam per .C. spatium .D. tempore, medietas ipsius .A. quae sit .E. movebit similter medietatem ipsius .B. quae sit .F. Nam similiter se habent haec secundum analogiam, id est, proportionem. Nempe qualis est proportio potentiae ad subduplam, talis est resistentiae ad resistentiam....

In tex. 37. excludit duas alias regulas, quas falso quis posset elicere ex quarta. Cum enim dictum sit, quod si aliqua potentia moveat resistentiam per aliquod spatium in aliquo tempore, subdupla movebit subduplam etc., posset quis inferre quod eadem potentia movebit duplam resistentiam, aut per dimidium spatium in eodem tempore, aut per idem spatium in duplo tempore, ut additio fiat ad resistentiam et non ad potentiam. Quae tamen regula exceptionem tunc patitur, quando multiplicata resistentia, potentiam moventis rei aequat vel superat, tunc enim potentia nullatenus movebit resistentiam, nec per aliquod spatium, nec in aliquo tempore. Quoniam a proportione aequalitatis vel minoris inaequalitatis non fit actio. Si tamen multiplicatio resistentiae non aequaret nec excederet potentiam, tunc regula vera quidem esset. Et est numero quinta....

Perseverat ergo in exemplo quartae regulae, quod si .E. medietas ipsius .A. movet .F. medietatem ipsius .B. non est necessarium, ut ipsa virtus .E. moveat totum .B. duplum ad .F. per aliquod spatium, quantunque parvum, in aliquo tempore quantuncumque magno, eadem ratione non est certa secunda regula, subtrahendo a potentia, et non a resistentia. Ut si .A. virtus (inquit philosophus) movet resistentiam .B. per aliquod spatium in aliquo tempore, non est necesse, ut .E. medietas ipsius .A. moveat totam resistentiam .B. per aliquod minus spatium aut in aliquo maiori tempore: quia poterit potentia tantum minui, ut adaequetur resistentiae aut superetur ab ea: tunc non poterit eam movere. Alias (inquit) homo possit movere navim, imo quantancumque magnitudinem. Nam si decem homines (verbi gratia) movere possunt totam navem per aliquod spatium aliquo tempore, tunc, si teneret proportio, posset unus homo movere totam navem, aut per subdecuplum spatium in eodem

tempore, aut in tempore decuplo per idem spatium. Si tamen potentia diminuta semper excedit resistentiam, vera est regula: est quae numero sexta.

Exempla harum regularum, quando verum habeant, quando vero minime, sic accipito. Si potentia ut quatuor movet resistentiam ut unum per aliquod spatium in aliquo certo tempore, eadem potentia movebit resistentiam ut duo per subduplum spatium in eodem tempore: sed tamen resistentiam ut quatuor nullatenus movebit: quoniam a proportione aequalitatis non fit actio. Pari ratione si potentia ut quatuor movet resistentiam ut unum per aliquod spatium in aliquo tempore, potentia ut duo movebit eandem resistentiam per subduplum spatium in eodem tempore: sed tamen potentia ut unum nullatenus movebit eam....

In tex. 38. subditur regula septima, a divisis ad coniuncta. Si duae sunt virtutes motivae, quarum seorsum singulae singulas resistentias scilicet pondera moveant, per aliquod spatium in aliquo tempore, virtus composita ex ambabus movebit aggregatum ex ambobus ponderibus per idem spatium in eodem tempore. Et ratio est, quia servatur semper proportio potentiae ad resistentias. Ut si singulae potentiarum sint ut duo, et singulae resistentiarum ut unum, ambae simul potentiae erunt ut quatuor, ambaeque resistentiae ut duo; atqui quatuor ad duo eadem est proportio, quae est duorum ad unum.

Translation:

[Text 35] ... From this, therefore, two rules are gathered. First, for the division of the thing moved and the multiplication of the distance, with the power and the time remaining equal: if any power moves any resistance through any distance in any time, the same or equal power will move half the resistance through double the distance in the same or equal time.[12] The reason is that whatever the ratio of half the resistance to the whole resistance, the same ratio will obtain between the distance and double the distance.

The second rule is for the division of the thing moved and the time, with the power and the distance remaining equal: if any power moves any resistance through any distance, the same or equal power will move half the resistance in half the time through an equal distance.[13] The proof: because whatever the ratio of the whole resistance to its half, the same ratio will obtain between the respective times.

[Text 36] ... The third rule is therefore for the division of the distance and the time, with the power and the distance remaining equal: if any power moves any resistance through any distance in any time, the same or equal power will move the same resistance through half the distance in half the time.[14]

The fourth rule is the converse, for the division of the power and the resistance with the distance and the time remaining equal: if any power moves any

[12] I.e., if A moves B over S in T, then T will move $B/2$ over $2S$ in T.
[13] I.e., if A moves B over S in T, then A will move $B/2$ over S in $T/2$.
[14] I.e., if A moves B over S in T, then A will move B over $S/2$ in $T/2$.

> resistance over any distance in any time, half the power will move half the resistance over the same space in the same time....[15]
>
> [Text 37] In text 37 he excludes two other rules, which one might falsely elicit from the fourth. For since it has been said that if any power moves a resistance through any distance in any time, one might infer that the same power will move double the resistance either through half the distance in the same time or through the same space in double the time, with an addition being made to the resistance and not to power. But then the rule admits of an exception, for when the resistance is multiplied such that it equals or exceeds the power, the power will not move the resistance over any distance or in any time, since no action results from a ratio of equality or lesser inequality. But if the multiplication of the resistance does not equal or exceed the power, the rule remains valid. And this is the fifth rule....[16]
>
> He continues then with another case of the fourth rule. If E, which is half A, moves F, which is half B, it is not necessary that E will move B, the double of F, over any distance however small and in any time however great; the same would apply to the second rule, if one were to diminish the power and not the resistance. For example, if power A (says the Philosopher) moves resistance B over any space in any time, it is not necessary that E, which is half A, will move the entire resistance B over any less distance or in any more time; for if the power is diminished such that it is equal to the resistance or is less than the resistance, it will not be able to move the resistance.... But if the diminished power always exceeds the resistance, the rule is valid. And this is the sixth rule.[17]
>
> In text 38 he adds the seventh rule, from separate to conjoined elements: if there are two motive powers each of which moves its respective resistance, say a weight, over any distance in any time, a power composed of both will move the aggregate of the two resistances or weights over the same distance in the same time.[18] And the reason is that the same ratio of power to resistances is always conserved. For example, if the individual powers are as 2, and the individual resistances as 1, both powers together will be as 4 and both resistances as 2; but 4 to 2 is the same ratio as 2 to 1.

SOTO'S *QUAESTIONES*: BOOK 7, QUESTION 4

Of the four questions that accompany Soto's commentary on the seventh book of Aristotle's *Physics*, only the last two are related directly to the so-called laws of motion. The first question inquires whether it is necessary

[15] I.e., if A moves B over S in T, then $A/2$ will move $B/2$ over S in T.

[16] I.e., if A moves A over S in T, then A will not move $2B$ over $S/2$ in T, nor will it move $2B$ over S in $2T$, in the event that $2B \geq$ A; otherwise it will do both.

[17] I.e., if A moves B over S in T, then $B/2$ will not move B over a shorter S in less T, unless $A/2 > B$, in which case the rule remains valid.

[18] I.e., if A moves B over S in T, and E moves F over S in T, then $A + E$ will move $B + F$ over S in T.

that mover and moved be in contact, and the second, whether any motion is always comparable to another. Following this, as already mentioned, comes the digression on ratios, and then the two questions dealing with the velocity of motions: the third, ascertaining it from its effect, and the fourth, from its cause. Here we supply the essentials of the last question, namely, whether or not this is determined by the ratio of the ratios (*proportio proportionum*) of the agent forces causing the motion to their respective resistances. As already indicated, the entire question is composed of forty-one paragraphs: the first seven of these pose various difficulties to be solved, in the style of a scholastic disputation; the eighth supplies the *sed contra*, which calls attention to Aristotle's seven rules as supplying a satisfactory answer; the ninth through the thirty-fifth give Soto's resolution of the question; and the last six, the thirty-sixth to the forty-first, resolve the difficulties noted at the outset. In what follows, the first seven and the last six paragraphs are omitted in order to save space, since they do not bear directly on Soto's resolution.

As previously, the Latin text that has been established is presented first and then, immediately following, its English translation. Notes are used in the Latin text to indicate variant readings, after the fashion of a critical apparatus. The term or expression for which there is a variant is given first, followed by a bracket, then the variant reading(s), and finally one or more capital letters designating the edition(s) listed in the second section above. Paragraphs are numbered throughout with the number of the paragraph as it is found in edition *C*. The same numbers are then used in the English translation, so as to permit easy access from the Latin to the English and vice versa. In the translation the notes serve a different purpose, providing a sort of commentary to the text in which different versions of the "laws" are put in modern notation, calculations are summarized, and other clarifications are provided. In the text of the translation, bracketed inserts provide the numerical equivalents of ratios given in Latin, so that the reader need not pause to look up terms such as "sesquialterate" and "double sesquitertian."

Latin text:

[8] In contrarium videntur agere septem regule Aristotelis, quas postremo capite huius libri statuendas curavit.

[9] Praenotare in primis[19] in quaestionis aditu, opus est quatuor esse (ut ait Aristoteles) consideranda in motu, scilicet id, quod movetur, et virtutem a qua, et tempus in quo, et spatium per quod movetur.

[19] in primis] imprimis *H*

[10] Porro autem virtus idem est que[20] virtus effectrix motus, sed id quod movetur est resistentia que a virtute motrice superanda est. Est enim regula hic supponenda, quod a proportione equalitatis aut minoris inequalitatis nulla fit actio, ut[21] lib. 2. dicere ceperamus, nempe, ubi resistentia sit equa aut maior quam virtus agentis, sed solum ubi proportio est maioris inequalitatis[22] agentis ad passum. Atque[23] tam virtus activa quam que resistit esse potest aut interne inherens mobili aut extrinsecus adiacens, ut dum grave cadit gravitas ipsa est interna virtus, et si quis illud expellat est extrinseca. Item medium ipsum per quod cadit, seu aqua sit seu aer, extrinseca est resistentia. In motibus itidem alterationis et augmentationis exempla etiam[24] sunt, sed que non sunt[25] presentis negotii. Preterea, virtus activa esse poterit aut spiritualis aut corporea, naturalis aut libera, fatigabilis et corruptibilis (ut in elementis et elementatis), atque infatigabilis et incorruptibilis, ut in corporibus celestibus. Titulus ergo questionis promiscue comprehendit omnes species, tam virtutis quam resistentie.

[11] Atqui varietas dignoscendi eiusmodi velocitatem motuum quatuor potest modis estimari. Primo, penes proportionem potentiarum motivarum. Secundo, penes proportionem inter resistentias. Tertio, penes proportionem inter excessus potentiarum super suas resistentias. Quarto, penes proportionem proportionum agentium super suas ipsarum resistentias.

[12] Exempli gratia, ut si sint .a. movens ut .8. et .b. movens ut .4. moventia pares resistentias ut .1, utrum .a. moveat iuxta primam regulam in duplo velocius quam .b, propterea quod activitas ut .8. est duplo maior quam[26] .4. An iuxta tertiam,[27] in proportione dupla sesquitertia, propterea quod excessus .8. ad .1. est .7. et .4. ad .1. est .3, et inter .7. et .3. est dupla sesquitertia. An vero secundum quartam, velocius moveat solum in sesquialtero, ex eo quod proportio octupla quae est inter .8. et .1. est sesquialtera ad quadruplum,[28] que est .4. ad .1, ut in proportionibus expositum est.

[13] Simili modo, si agens .a. virtutis ut .4. moveat resistentiam ut .2, et .b. eiusdem virtutis ut .4. moveat resistentiam ut .3, utrum .a. iuxta secundam regulam moveat in sesquialtero velocius quam .b, ideo quod resistentia ut .2. est in sesquialtero minor quam resistentia ut .3; an secundum tertiam, in duplo velocius, propterea quod excessus .4. ad .2. est duplus ad excessum .4. ad .3. An vero iuxta quartam, plusquam in duplo velocius, idcirco[29] quod proportio dupla .4. ad .2. est maior quam dupla ad proportionem sesquitertiam, que est .4. ad .3.

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[20] que] quod *J*
[21] ut] et ut *H*
[22] inequalitatis] inequalitates *EF*
[23] atque] atqui *J*
[24] etiam] enim *F*, om. *J*
[25] que non sunt] non *J*
[26] quam] quam ut *J*
[27] iuxta tertiam] taxitertiam *EF*
[28] quadruplum] quadruplam *FJ*
[29] idcirco] id circo *BDEG*

[14] Porro autem, in prima et in secunda regula perquam[30] tenuis est probabilitas, sed inter tertiam et quartam relinquitur disceptatio, et tandem quinta est ex mente Aristotelis omnino amplectenda.

[15] Respondetur ergo ad quaestionem quatuor conclusionibus. Prima est adversus primam regulam. Velocitas motus non est attendenda penes proportionem virtutis unius agentis ad virtutem alterius. Conclusio in primis patet, quia ubi resistentie essent[31] inequales, nulla esset apparentia solam esse expectandam[32] proportionem inter virtutes motrices. Quis enim diceret quod si virtus ut .6. moveat resistentiam ut .1. et virtus ut .4. resistentiam ut .2, non moveat virtus ut .6. nisi in sesquialtero velocius (que est proportio, quam habet ad .4.), cum ad suam resistentiam habeat sextuplam .4. vero ad suam, non nisi duplam.

[16] Sed tamen, et ubi resistentia sit eadem conclusio nihilominus est notissima; alias, si virtus ut .4. moveret resistentiam ut .3. aliqua velocitate, virtus ut .2. subdupla ad .4. moveret eandem resistentiam subdupla velocitate, quod est impossibile, cum a proportione minoris inequalitatis nulla sit[33] actio. Qua de causa ait Aristoteles text. 37. quod non quotiescunque aliqua virtus movet aliquam resistentiam in aliquo tempore, subdupla movebit eandem in duplo[34] tempore.

[17] Miror tamen quosdam schole nostre qui aiunt regulam primam, contrariam huius conclusionis,[35] veram esse in universum, posita constantia potentie motive. Quod est dicere, fixa eadem resistentia, que sit minor utraque potentia. Existimant enim idem esse tunc metiri velocitates aut penes primam regulam, aut certe penes quartam, id quod est manifeste falsam. Nam si virtus ut .8. moveat resistentiam ut .1, et virtus ut[36] .4. moveat eandem resistentiam, velocitas motus prioris ad eam que est posterioris, secundum primam regulam habebit proportionem duplam, qualis est .8. ad .4, et secundum quartam habebit tantum sesquialteram, qualis est octuple proportionis ad quadruplam. Continet enim octupla quadruplam, et item duplam, que est medietas quadruple.

[18] Secunda conclusio contra secundam regulam. Neque velocitas motus attendenda est penes proportionem resistentiarum, que quidem conclusio eisdem penitus rationibus constat quibus secunda. Ubi nanque diverse sunt virtutes active, palam peccat secunda regula. Nulla enim est apparentia, quod ubi resistentia ut .2. movetur a potentia ut .6. et resistentia ut .3. a potentia ut .4, non moveatur prima resistentia velocius quam secunda, nisi in sesquialtero, qualis est proportio inter resistentias, sane cum prima moveatur a proportione tripla, et secunda nonnisi[37] a sesquitertia.

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[30] perquam] per quam *CH*
[31] essent] essent sent *G*
[32] expectandam] spectandam *J*
[33] sit] fit *E*
[34] duplo] subduplo *J*
[35] huius conclusionis] huic conclusioni *J*
[36] ut] om. *EF*
[37] nonnisi] non, nisi *BCDEFG*

[19] Neque ubi virtus est eadem habet veram, alias si resistentia ut .2. moveretur a virtute ut .4. aliqua proportione, sequeretur quod resistentia ut .4, que est dupla, moveretur ab eadem virtute subdupla velocitate. Quod est falsum, eo quod a proportione equalitatis non sequitur actio.

[20] Neque si virtus utranque vinceret resistentiam, certa esset regula. Nam si resistentia ut .1. moveretur a virtute ut .8. et resistentia ut .4. ab eadem ipsa, tunc per secundam regulam velocitas prioris motus deberet excellere velocitatem posterioris quadrupla proportione, que est inter resistentias; et tamen secundum quartam, que est omnium norma, non excellit nisi proportione tripla, qualis est octuple proportionis (quam virtus habet super priorem resistentiam) ad proportionem duplam, quam habet ad posteriorem. Componitur enim octupla proportio ex tribus duplis.

[21] Tertia conclusio adversus tertiam regulam, in qua plusculum est difficultatis. Velocitas motus ex parte cause non attenditur penes proportionem arithmeticam, que est inter excessus virtutum[38] super suas singularum resistentias. Conclusio hec demonstrationem fortitur[39] ex quarta regula Philosophi tex. 36. Enimvero si regula[40] tertia, huic conclusioni contraria, veridica[41] esset, consequens fieret, ut si .a. virtus ut .8. moveret resistentiam ut .4. et .b. virtus ut .2. moveret resistentiam ut .1, velocitas primi motus esset velocitate secundi quadruplo maior, qualis est proportio excessus prime potentie super suam resistentiam ad excessum secunde super suam, scilicet .4. ad .1. Cuius tamen contrarium astruit dicta regula Aristotelis quarta que[42] talis est. Si aliqua virtus moveat aliquam resistentiam in aliquo tempore per aliquod spatium, subdupla virtus movebit subduplam resistentiam eodem tempore per idem spatium, et ita deinceps, dividendo virtutem et resistentiam. Quo fit, ut virtus ut .2. eadem velocitate moveat resistentiam ut .1. qua virtus ut .8. resistentiam ut .4. Et huius causa, illud est, quod utraque virtus movet suam resistentiam a proportione dupla.

[22] Hinc manifeste colligitur quarta conclusio, qua stabilitur quinta regula. Velocitas motus penes causa attenditur penes proportionem proportionum agentium super suas ipsorum resistentias. Que quidem proportio (ut tractatu de proportionibus prefati sumus) dicitur proportionalitas geometrica. Conclusio est, quam Paulus Venetus, Hentisberus, et fere ennatores Aristotelis consentienter affirmant. Nam etsi nusquam fuerit ab Aristotele sub his terminis constituta, colligi eam tamen existimant ex suis regulis septem.

[23] Primum ex quarta manifeste (ut modo dicebamus), eo enim quod qualis est proportio virtutis cuiuspiam ad eam quam superat resistentiam, talis est medietas virtutis ad medietatem resistentie, docet eadem velocitate movere subduplam virtutem, subduplam resistentiam, qua tota movebat totam. Namque ait eodem tempore per idem spatium eandem movere.

[38] virtutum] virtutem *DEFG*
[39] fortitur] sortitur *CF*
[40] Philosophi tex. 36. Enimvero si regula] om. *H*
[41] veridica] veri dica *BE*
[42] quarta que] quarta, que *BH*; et quarta que *J*

[24] Sequitur item eadem quarta conclusio dilucide ex septima regula. Nempe, quod si due virtutes seorsum moveant aliquo uno tempore duas resistentias, virtus composita ex ambabus virtutibus movebit resistentiam compositam ex ambabus resistentiis eodem tempore per idem spatium, quod est dicere eadem velocitate. Cuius profecto ratio est, quam asserit quarta conclusio. Nempe, quod talis est proportio ambarum simul virtutum ad ambas simul resistentias, qualis est alterutrius ad suam. Loquitur enim de virtutibus habentibus easdem proportiones ad suas resistentias, siquidem ait movere easdem eodem tempore. Etenim, si virtus ut .6. moveat resistentiam ut .4. et virtus ut .3. resistentiam ut .2, tunc virtus ut .9, que constat ex .6. et .3, movebit resistentiam ut .6, que constat ex .4. et .2, eadem velocitate. Quoniam omnes sunt proportiones sesquialtere. Et idem comperies in omni genere proportionis.

[25] At vero contra istam conclusionem, et pro examine regularum Aristotelis, arguitur primo authoritate eiusdem Aristotelis primo de celo, cap. de infinito, existimantis velocitatem motuum attendi penes excellentiam moventis ad mobile. Et idem videtur sentire Commentator, lib. 4. tex. 70. et lib. hoc .7. tex. 35.

[26] His tamen locis facile responsum dedimus,[43] Aristotelem solum affirmare, quod quanto excellentia erit maior, tanto motus erit celerior; nihil tamen loquitur de proportionibus, quod qualis fuerit proportio excessus ad excessum, talis sit velocitas[44] ad velocitatem. Et Commentator tex. hic .36. videtur se ipse[45] exponere, ut bene agnovit Paulus, scilicet, quod per excessum intelligebat proportionem. Et sic, comparando inter se duo agentia, comparat proportionem unius excessus super suam resistentiam proportioni alterius supra suam. Sed aliter urgentius sic arguitur.

[27] Etsi quarta nostra conclusio optime consonet[46] quarte regule Aristotelis, reliquis vero non similiter. Est nanque prima regula, quod si aliqua virtus moveat resistentiam in aliquo tempore per aliquod spatium, eadem virtus eodem tempore movebit subduplam resistentiam per duplum spatium, id est, dupla velocitate. Et quidem particulariter, ubi virtus movet resistentiam dupla proportione, consentit quarta conclusio. Etenim si virtus ut .8. moveat resistentiam ut .4. aliquo tempore per aliquod spatium, eadem virtus movebit resistentiam ut .2. in duplo velocius, quia proportio quam .8. habet ad .2. est dupla ad duplam quam habebat ad .4.

[28] Et per hoc item refutatur tertio iam impugnata regula de excessibus. Qui si essent hic aspiciendi, solum excederet velocitas in sesquialtero, qualis est proportio excessus quo .8. excedit .2. ad excessum quo excedit .4. At vero non subinde sequitur (salua quarta conclusione) quod eadem virtus eodem tempore moveat subquadruplam resistentiam per quadruplum spatium, id est, quadrupla velocitate, sed solum movebit in triplo velocius, qualis est proportio octuple, scilicet, .8. ad .1, ad duplam, puta .8. ad .4.

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[43] responsum dedimus] responsumimus *B*
[44] velocitas] velocitatis *J*
[45] se ipse] seipsum *J*
[46] consonet] consonent *DEHJ*

[29] Qua ratione colligi videtur, quod vel quinta nostra conclusio est falsa, vel prima regula Aristotelis. Respondebitur forsan, quod regula Aristotelis solum loquitur de medietate resistentie, non tamen subinde procedit ad minores partes.

[30] Sed et contra hoc etiam nunc[47] pugnat aperte replica. Si virtus motiva ut .6. moveat in aliquo tempore resistentiam ut .4, eadem virtus non movebit medietatem (iuxta quartam conclusionem) eodem tempore per duplum precise spatium, quia non movebit solum duplo maiori velocitate; ergo quarta conclusio repugnat Aristoteli. Probatur antecedens: .6. ad .2. est proportio tripla, ad quatuor vero est sesquialtera; tripla autem ad sesquialteram est maior quam dupla. Constat enim ex duabus sesquialteris et una sesquitertia, ut tripla que est .12. ad .4.[48] constat ex sesquitertia, que est .12. ad .9, et sesquialtera que est .9. ad .6, et sesquialtera que est .6. ad .4;[49] ergo virtus ut .6.[50] plusquam duplo velocius movebit resistentiam ut .2.[51] quam resistentiam ut .4.

[31] Quinimo est regula generalis, quod quandocunque virtus excellit resistentiam minori proportione quam dupla, eadem virtus habebit ad medietatem resistentie maiorem proportionem quam duplam, et per consequens movebit eam plusquam duplo velocius. Et econverso quandocunque[52] virtus exuperat resistentiam maiori proportione quam dupla, excedit medietatem resistentie minori quam dupla. Ut si virtus ut .6. moveat resistentiam ut .2. aliqua velocitate, non movebit resistentiam ut .1. duplo maiori, sed citra. Quoniam .6. ad .1. habet proportionem sextuplam,[53] ad .2. vero triplam, et sextupla ad triplam est minus[54] quam dupla. Constat enim ex dupla et tripla, quare dupla non est eius medietas.

[32] Eodem omnino modo patescit, eandem conclusionem quartam non in universum consentire cum .2. regula Aristotelis, que venit in idem cum prima, videlicet, si aliqua virtus movet resistentiam in aliquo tempore per aliquod spatium, eadem virtus movebit subduplam resistentiam per idem spatium subduplo tempore, quod pariter est dicere, dupla velocitate. Quare si quartam conclusionem sustineas, nequibus verificare regulam nisi ubi virtutis ad resistentiam fuerit iuste proportio dupla.

[33] Tertia vero, et quinta, et sexta regule Aristotelis nihil habent hic peculiaris difficultatis.

[34] Ad hec igitur argumenta ferme Calculatores nihil aliud respondent, quam quod prima et secunda regula duntaxat habet verum, ubi virtutis ad resistentiam est proportio dupla, neque maior, neque minor. Nam tunc ad medietatem erit quadrupla, que est dupla ad duplam. Et certe ita necessarium est dicere.

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[47] etiam nunc] etiamnum *B*
[48] .4.] quarta *F*
[49] .4;] quartam *F*
[50] .6.] sextam *F*
[51] .2.] secundam *F*
[52] quandocunque] quandoque *H*
[53] sextuplam] suxtuplam *DEG*
[54] minus] minor *J*

Nam in aliis proportionibus,[55] neque si velocitas attendatur penes proportionem proportionum, ut dicit quarta conclusio, et multo minus si attendatur penes proportionem inter excessus, possunt defensari eiusmodi regule.

[35] Attamen ne Aristoteles videatur tam mancus qui in solo casu particulari constituerit regulas. Et preterea, ut quarta nostra conclusio sit universalis, interpretande sunt dicte regule, quod nomine medietatis resistentie intelligit illam generaliter, ad quam virtus habet duplam proportionem ad proportionem quam habet ad totam resistentiam. Ut si virtus ut .6. moveat resistentiam ut .4. aliqua velocitate, eadem virtus movebit subduplam resistentiam, id est, illam super quam habebit proportionem duplam ad sesquialteram dupla velocitate, et per consequens, eodem tempore per duplum spatium, aut per idem spatium subduplo tempore. Proportio autem dupla ad sesquialteram est maior quam dupla, puta dupla sesquiquarta, qualis est .9. ad .4. Et pari ratione eadem virtus movebit tertiam partem resistentie (id est, illam, super quam virtus habet proportionem triplam ad proportionem quam habet[56] super totam resistentiam) velocitate triplo maiorem. Huiusmodi arte patere cuicunque potest via dignoscendi velocitates motus.

Translation:

[8] On the contrary there seem to stand the seven rules of Aristotle, which he saw fit to put in the last chapter of this Book.

[9] First, when approaching this question, one must consider the four elements involved in motion (as Aristotle says), namely, the thing moved, the power that moves it, the time during which it moves, and the space over which it moves.

[10] The power is the same as the power that effects the motion, while the thing moved is the resistance that is overcome by the motive power. Here a rule is to be presupposed, that there is no action from a ratio of equality or of lesser inequality, as we undertook to point out in Book 2, namely, when the resistance is equal to or greater than the power of the agent, but only when the ratio between agent and patient is one of greater inequality. Both the active power and the resistive force can be something either inhering internally in the moving object or coming to it from without, as when a heavy object falls its gravity is an internal power, and, if someone should throw it, this is extrinsic. Again, the medium through which it falls, whether this be water or air, is an extrinsic resistance. Examples are also available in the motions of alteration and of augmentation, but these do not pertain to the present inquiry. Again, the active power can be either spiritual or corporeal, natural or free, fatigable and corruptible (as in the elements and compounds) or indefatigable and incorruptible, as in the heavenly bodies. Thus the title of the question takes in all kinds of powers as well as resistances, indifferently.

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[55] Nam in aliis proportionibus] om. *J*

[56] proportionem triplam ad proportionem, quam habet] om. *CDEFGHJ*

[11] There are four ways of looking at the different ways of judging the velocities of the motions of such objects. First, from the ratio between the motive powers;[57] secondly, from the ratio between the resistances;[58] thirdly, from the ratio between the excesses of the powers over the resistances;[59] fourthly, from the ratio of the ratios of the agents to their corresponding resistances.[60]

[12] For example, if movers *A* of 8 and *B* of 4 move equal resistances of 1, does *A* move with twice the velocity of *B* because its activity, being 8, is twice as great as 4, according to the first rule? Or, following the third rule, in a double sesquitertian ratio [i.e., 7/3], because the excess of 8 over 1 is 7 and that of 4 over 1 is 3, and between 7 and 3 there is a double sesquitertian? Or, following the fourth rule, would *A* move with a velocity that is greater only in a sesquialterate ratio [i.e., 3/2], because the octuple ratio that holds between 8 and 1 is sesquialterate with respect to the quadruple that holds between 4 and 1, as has been explained in the digression on ratios?[61]

[13] Similarly, if agent *A*, with a power of 4, moves a resistance of 2, and *B*, with the same power of 4, moves a resistance of 3, would *A*, according to the second rule, move with greater velocity than *B* in a sesquialterate ratio [i.e., 3/2], because a resistance of 2 is in a sesquialterate ratio less than a resistance of 3;[62] or, according to the third rule, with twice the velocity, because the excess of 4 over 2 is twice the excess of 4 over 3?[63] Or, according to the fourth rule, with more than double the velocity, from the fact that the ratio of 4 to 2 doubled is more than the sesquitertian ratio of 4 to 3 doubled?[64]

[14] Further, there seems to be but slight probability in the first and second rules, while the third and fourth leave room for debate; and, finally, according to the mind of Aristotle, a fifth[65] is to be completely accepted.

[15] The reply to the question may be made, therefore, under four conclusions. The first is against the first rule. The velocity of motion is not to be ascertained from the ratio of the power of one agent to the power of the other.

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[57] I.e., $V_2/V_1 = F_2/F_1$, presupposing that $R_2 = R_1$.
[58] I.e., $V_2/V_1 = R_1/R_2$, presupposing that $F_2 = F_1$.
[59] I.e., $V_2/V_1 = (F_2 - R_2)/(F_1 - R_1)$.
[60] This statement permits of two formulations, depending on the sense of the expression "ratio of ratios" (*proportio proportionum*). If "ratio" is to be understood as a simple arithmetical ratio, then the meaning is $V_2/V_1 = (F_2/R_2)/(F_1/R_1)$—that is, the ratio of the respective ratios of the forces to the resistances. If "ratio" is to be understood in Bradwardine's sense as a geometrical or exponential ratio, then the meaning is more complex. Expressed in words it means that V_2/V_1 is the exponent to which F_1/R_1 must be raised to equal F_2/R_2. Stated algebraically, this becomes $(F_2/R_2) = (F_1/R_1)^{V_2/V_1}$ an expression that is termed "Bradwardine's function." From the example Soto gives at the end of par. [12], formulated in the next note, it is clear that he intends the second meaning here, not the first.
[61] I.e., $8/1 = (4/1)^{3/2}$.
[62] I.e., $3/(3/2) = 2$.
[63] I.e., $(4 - 2) = 2(4 - 3)$.
[64] I.e., with an exponent of two, since $(4/2)^2$ or 16/4 is greater than $(4/3)^2$ or 16/9.
[65] Note the reference to a "fifth" conclusion, which is not explicitly indicated in Soto's fourth question, but which is mentioned again in pars. [22] and [29].

This conclusion is apparent, first of all, because in the case where the resistances are unequal there is no reason why one should take account only of the ratios between the motive powers. For, should one say that if a power of 6 were to move a resistance of 1 and a power of 4 a resistance of 2, a power of 6 would move with greater velocity only in a sesquialterate ratio [i.e., 3/2]—which is the ratio of 6 to 4—when 6 would have a sextuple ratio to its resistance [i.e., 6/1] and 4 only a double ratio to its [i.e., 4/2 = 2]?

[16] In the case where the resistances are the same, on the other hand, the conclusion is still obvious; otherwise, if a power of 4 were to move a resistance of 3 with a given velocity, a power of 2, the half of 4, would move the same resistance with half the velocity, and this is impossible, because there cannot be any action on the basis of a ratio of lesser inequality. For this reason Aristotle says in text 37 that it is not true that if a certain power can move a resistance in a given time, half the power will move the same in twice the time.

[17] I marvel, however, at certain members of our school, who say that the first rule, contrary to this conclusion, is true universally, granted the constancy of the motive power, that is to say, making the resistance the same but less than either power. For they think that determining the velocity by the first rule is the same as by the fourth, and this is obviously false. For, if a power of 8 moves a resistance of 1, and a power of 4 moves the same resistance, the velocity of the first motion to the second would be in double ratio, that of 8 to 4, according to the first rule,[66] whereas according to the fourth, it would be only in a sesquialterate ratio, that of the octuple ratio to the quadruple, for an octuple contains a quadruple and a double again, which is half the quadruple.[67]

[18] The second conclusion is against the second rule. For the velocity of motion is not to be ascertained from the ratios between the resistances, and this conclusion follows from the same reasons as the first. For the second rule is clearly inapplicable where there are different agent powers. Thus there is no reason why, if a resistance of 2 is moved by a power of 6 and a resistance of 3 by a power of 4, the first resistance is not moved with a greater velocity than the second, and in a sesquialterate ratio, which is the ratio between the resistances [i.e., 3/2]; and this because the first is moved with a triple ratio [i.e., (6/2) = (3/1)] and the second is not, but rather with a sesquitertian [i.e., 4/3].

[19] Nor is the rule true when the power is the same, for if a resistance of 2 were moved by a power of 4, it would follow from the ratio that a resistance of 4, which is double [i.e., twice two], would be moved by the same power with half the velocity. And this is false, by the fact that no action proceeds from a ratio of equality.

[20] Nor would the rule be correct if the power were sufficient to overcome both resistances. For, if a resistance of 1 were moved by a power of 8 and a

[66] I.e., $(8/1)/(4/1) = 8/4 = 2/1$.
[67] I.e., $(8/1) = (4/1)(2/1) = (4/1)^{3/2}$.

resistance of 4 by the same, then, according to the second rule, the velocity of the first motion should exceed the velocity of the second by the quadruple ratio that obtains between the resistances [i.e., 4/1]; yet, according to the fourth rule, which is the best norm, it would not exceed by more than a triple ratio, which is that of the octuple ratio (which the power has over the first resistance) to the double ratio it has over the second. For an octuple ratio is composed of three doubles.[68]

[21] The third conclusion is against the third rule, and in this there is somewhat more difficulty. The velocity of motion is not ascertained on the part of the cause from the arithmetical proportion that exists between the excesses of the powers over their respective resistances. This conclusion gets its demonstration from the fourth rule of the Philosopher, text 36. For if the third rule, which is contrary to this conclusion, were true, there would result as a consequence that if *A*, with a power of 8, were to move a resistance of 4, and *B*, with a power of 2, were to move a resistance of 1, the velocity of the first motion would be four times greater than the velocity of the second, for this is the ratio of the excess of the first power over its resistance [i.e., $8 - 4 = 4$] to the excess of the second over its [i.e., $2 - 1 = 1$], namely, 4 to 1. Contrary to this, however, one may oppose Aristotle's fourth rule, already mentioned, which is the following: if any power were to move any resistance in any period of time over any space, half the power would move half the resistance in the same time over the same space, and so on, dividing both the power and the resistance. From this it follows that a power of 2 would move a resistance of 1 with the same velocity as a power of 8 would move a resistance of 4 [since $(2/1) = (8/4)$]. And the reason for this is that each power moves its resistance in a double ratio [i.e., 2/1].

[22] From the foregoing the fourth conclusion is easily deduced, and with this the fifth[69] rule is established. The velocity of motion ascertained from its cause is determined by the ratio of the ratios of the agents to their resistances. And this ratio (as we have already said in the treatise on ratios) is said to be a geometrical ratio. The conclusion is one that Paul of Venice,[70] Heytesbury,[71] and practically all the commentators on Aristotle commonly affirm. For, even if Aristotle never did state this in these terms, nevertheless they feel that one can deduce it from his seven rules.

[23] This obviously follows, first, from [Aristotle's] fourth rule (as we were just saying), from the fact that the same ratio exists between any power and

[68] I.e., $(8/1) = (2/1)(2/1)(2/1) = (2/1)^3$.

[69] Note again the reference to a "fifth" rule, which has already been mentioned in par. [14] and will recur again in par. [29].

[70] See Paul of Venice, *Summa naturalium, Physica* (Venice, 1503), cap. 32, fol. 17vb: "Quarta conclusio. Velocitas in motu locali attenditur penes proportionem proportionum potentiarum moventium ad suas resistentias: ita quod ex equalibus proportionibus potentiarum moventium ad suas resistentias provenit equalis velocitas; ex inequalibus inequalis. scilicet. ad duplam dupla velocitas: et ad subdublam subdupla velocitas: ut si a. ad b. esset proportio quadrupla: et c. ad d. esset proportio dupla: dico quod a. moveret b. in duplo velocius quam c. d. eo quod proportio quadrupla est dupla duple."

[71] The reference is to William Heytesbury (fl. c. 1355), the Oxford calculator, but no indication is given of the work being referenced.

the resistance it overcomes as exists between half the power and half the resistance, and thus half the power will move half the resistance with the same velocity as the entire power moves the whole resistance. For he says that it moves the same resistance over the same space in the same time.

[24] The fourth conclusion also follows clearly from the seventh rule, namely, that if two powers were separately to move two resistances in any one time, the power composed of the two powers would move a resistance composed of both resistances in the same time over the same space, that is to say, with the same velocity. And the reason for this is the same as is asserted in the fourth conclusion, namely, that the same ratio exists between both powers taken together and both resistances taken together as exists between each taken separately. For to speak of powers as having the same ratios to their resistances is to say equivalently that they will move them in the same time. Thus, if a power of 6 moves a resistance of 4, and a power of 3 a resistance of 2, then a power of 9, which is composed of 6 and 3, will move a resistance of 6, which is composed of 4 and 2, with the same velocity. The reason is that all are sesquialterate ratios [i.e., $(9/6) = (6/4) = (3/2)$]. And the same will be found to be true in every other kind of ratio.

[25] But, against this conclusion, and to give closer examination to Aristotle's rules, there is the argument first from the authority of Aristotle himself, in the first book of *De caelo*, in the chapter on the infinite, where he proposes that the velocities of the motions should be ascertained from the superiority of the mover over the thing moved. And the same seems to be the opinion of the Commentator[72] in Book 4, text 70, and here in Book 7, text 35.

[26] But to these texts we can easily reply that Aristotle intended to state only that the more the superiority the greater the velocity; "more quickly" says nothing of ratios, nor does it say that the ratio of one excess to the other excess is that of one velocity to the other. Yet the Commentator himself, here in text 36, seems to say, as Paul [of Venice] properly recognized, that by excess he meant ratio.[73] And so, comparing two agents to each other, he compares the ratio of the excess of one over its resistance to the ratio of the other over its. But it seems that one can argue more cogently in a different manner as follows.

[27] Although our fourth conclusion agrees very well with Aristotle's fourth rule, the same cannot be said for the others. For the first rule is that, if some power moves a resistance over a given space in a particular time, the same power will move half the resistance over twice the space in the same time, that is, with double velocity. And, in this particular case, when the moving power and the resistance stand in the ratio of 2 to 1, the fourth conclusion is in agreement. For, if a power of 8 moves a resistance of 4 over a given space in a particular time, the same power will move a resistance of 2 with double the velocity, because the quadruple ratio that exists between 8 and 2 [i.e., $(8/2) = (4/1)$] is double the double ratio that exists between 8 and 4 [since $(8/2) = (8/4)^2$].

[72] That is, Averroes (1126–98), in his great commentary on the *Physics*.

[73] Paul of Venice, *Summa naturalium* (n. 70).

[28] In this way the third rule concerning excesses, which has already been disproved, is likewise refuted. For, if the excesses were here taken into account, the velocity would be greater only in a sesquialterate ratio, which is the ratio of the excess by which 8 is greater than 2 to the excess by which it is greater than 4.[74] But from this it does not immediately follow (independently of the fourth conclusion) that the same power would move a quarter the resistance four times the distance in the same time, i.e., with four times the velocity, but that it would move it only with three times the velocity, for this is the ratio of the octuple, namely, 8 to 1, with respect to the double, namely, 8 to 4.[75]

[29] From this argument it would seem to follow that either our fifth[76] conclusion, or the first rule of Aristotle, is false. Perhaps the reply to this is that the rule of Aristotle speaks only of half resistances, and that one cannot apply it directly to smaller parts.

[30] Now, however, another objection obviously militates even against this. If a motive power of 6 were to move a resistance of 4 in a certain time, the same power would not move half the resistance (according to the fourth conclusion) over exactly twice the distance in the same time, because the velocity would not be simply double; therefore the fourth conclusion is opposed to Aristotle. The antecedent is proved as follows: 6 to 2 is a triple ratio [i.e., 3/1], whereas 6 to 4 is sesquialterate [i.e., 3/2]; but the ratio of a triple ratio to a sesquialterate ratio is more than double.[77] For a triple ratio is composed of two sesquialterates and one sesquitertian,[78] just as the triple, 12 to 4, is composed of a sesquitertian, 12 to 9, and a sesquialterate, 9 to 6, and a sesquialterate, 6 to 4.[79] Therefore a power of 6 will move a resistance of 2 with more than twice the velocity it will move a resistance of 4.[80]

[31] There is the general rule, in fact, that, whenever a power exceeds a resistance in a ratio less than double, the same power will have a ratio to half the resistance greater than double the first ratio, and as a consequence it will move the resistance with more than twice the velocity. And, vice versa, whenever a power exceeds a resistance by a ratio greater than 2 to 1, it will exceed half the resistance by less than double the original ratio. Thus, if a power of 6 moves a resistance of 2 with a given velocity, it will move a resistance of 1, not with double the velocity, but with less. For 6 stands in sextuple ratio to 1, and in triple ratio to 2, and the ratio of the sextuple to the triple is less than double.[81] For the sextuple is composed of a double and a triple, and for this reason the double is not its half.[82]

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[74] I.e., $(8-2)/(8-4) = (6/4) = (3/2)$

[75] Since $(8/1) = (8/4)(8/4)(8/4) = (8/4)^3$.

[76] Note yet again the reference to a "fifth" conclusion, previously mentioned in pars. [22] and [29].

[77] I.e., $(3/1)$ is greater than $(3/2)(3/2)$, which is equal to $(3/2)^2$, or 2 1/4.

[78] I.e., $(3/1) = (3/2)(3/2)(4/3)$.

[79] I.e., $(12/4) = (12/9)(9/6)(6/4)$.

[80] Since $(6/2) > (6/4)^2$, i.e., $3 > 36/16$, or 2 1/4.

[81] I.e., 6 or $(6/1)$ is less than $(3/1)(3/1)$, which is equal to $(3/1)^2$, or 9.

[82] I.e., $(6/1) = (2/1)(3/1)$ and $(6/1)^{1/2} \neq (2/1)$.

[32] And in exactly the same manner it becomes obvious that the fourth conclusion itself does not agree completely with the second rule of Aristotle, which agrees with the first, namely, if a given power moves a resistance over a determinate distance in a particular time, the same power will move half the resistance over the same distance in half the time, which is the same as to say with twice the velocity. For this reason, if one subscribes to the fourth conclusion, one cannot verify the rule, except in the case where the ratio of the power to the resistance is exactly double.

[33] The third, and the fifth, and the sixth rules of Aristotle here do not offer any special difficulty.

[34] To these arguments, therefore, nearly all the Calculators[83] say no more than that the first and second rules are true only when the ratio of the power to the resistance is double, and neither more nor less. For then the ratio to half will be quadruple, which is double with respect to double [i.e., $(4/1) = (2/1)^2$]. And certainly it is necessary to say this. For with other ratios, if the velocity is determined from the ratio of the ratios, as the fourth conclusion states, and even less if it is determined from the ratio between the excesses, these rules cannot be defended.

[35] Nevertheless, Aristotle should not be thought deficient for having formulated rules that are valid only in a special case. Moreover, since our fourth conclusion is universal, the foregoing rules are to be interpreted in such a way that by the expression "half the resistance" he means generally that to which the power has a ratio double the ratio it has to the entire resistance. Thus, if a power of 6 moves a resistance of 4 with a given velocity, the same power will move half the resistance (that is, that over which it will have a ratio double a sesquialterate ratio) with twice the velocity, and, as a consequence, it will move over twice the distance in the same time, or over the same space in half the time. A ratio double a sesquialterate ratio is more than a ratio of 2 to 1—it is a double sesquiquartan ratio, i.e., 9 to 4.[84] And, for the same reason, the same power will move one third part of the resistance (i.e., that over which the power has triple the ratio it has to the entire resistance) with a triple velocity.[85] And with a technique of this kind the way is clear for anyone to be able to ascertain the velocities of motions.

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[83] This term is applied onomastically to Richard Swineshead or Suiseth (fl. c. 1340–55), who composed a *Liber calculationum* while at Merton College, Oxford; it is also used generally to refer to Bradwardine, Heytesbury, and others of that school who were adept at *calculationes* of the type being considered here.

[84] Since $(3/2)^2 = (3/2)(3/2) = 9/4$.

[85] The calculation Soto is proposing here, when his parenthetical expression is taken into account, would read $(3/2)^3 = (3/2)(3/2)(3/2)$, and this yields a ratio of 27/8. What this means is that an F/R ratio of 27 to 8 would be required to produce a velocity three times as fast as that produced by an F/R ratio of 3 to 2. So, if the force stays at 3, a resistance of 8/9 might be thought of as "one third part" of the resistance of 2, because the ratio of 3 to 8/9 is "three times" (i.e. the cube of) the ratio of 3 to 2. Alternatively, to take an example that involves only whole numbers, if a force of 8 moves a resistance of 4 with a given velocity, then a resistance of 1 will be called a resistance one-third of the resistance of 4 because the ratio 8/1 is "three times" (i.e. the cube of) the ratio 8/4. If so, the force of 8 will move the resistance of

In the Latin text, as has already been remarked, the variant readings are designated by note numbers 19 through 56, totaling 38 in all. (Only 10 of these variants, it should be noted, were included in the 45 soundings taken from all four questions on Book 7 to trace the provenance of the nine editions, but this is of no importance for what follows.) It turns out that few of the 38 variants are significant for determining the sense of Soto's exposition. The most significant is the last, note 56, which shows an omission of six words from the parenthetical expression in par. [35] that otherwise would clarify what Soto means by "one third part of the resistance," a point to be discussed later. Also important are the variants in notes 48 through 51 in par. [30], which show that edition *F* is flawed in its numerical examples, giving ordinal numbers in Latin for what were intended to be cardinal numerals. And it should be noted that the three occurrences of "fifth" (*quinta*) referring to a fifth "rule" or "law" in pars. [14], [22], and [29] show no variant readings, which indicates that no editor questioned the existence of such a law even though it is not stated explicitly in the text.

There are obviously difficulties in the foregoing text, the main one relating to the number of conclusions Soto is offering and precisely how his "fifth" rule or law, whatever it may be, is related to Bradwardine's "ratio of ratios" as this is invoked to explain the ratios of velocities produced by various powers or forces working against different resistances. One should consider, however, that the question in which the text is situated is really part of Soto's commentary on Aristotle's seventh book, in which he, like Bradwardine before him, was intent on improving Aristotle's seven rules and indeed assimilating them, as well as Averroes's teachings, within the new formulations. (Note that at the first occurrence of the "fifth" rule, par. [14], Soto does present this as being "according to the mind of Aristotle.") Going back and forth between Aristotle's seven rules and his own four or five understandably could have occasioned some confusion in Soto's exposition. However that may be, there can be no doubt that Soto understood Bradwardine's geometrical or exponential ratios correctly, as can be seen from the examples he provides in pars. [12], [13], [17], [20], [27], [28], [30], [31], [34], and [35].[86]

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1 three times as fast as it moves the resistance of 4, since $8/1 = (8/4)(4/2)(2/1) = (2/1)^3$. Edith Sylla, who has proposed this interpretation to me, observes that this would be a clever way of "reading" Aristotle to agree with Bradwardine. And, as long as Aristotle never gives examples except those involving ratios of 2/1, no glaring counterinstances would rule out this "interpretation."

[86] The same cannot be said of the Augustinian Alonso de la Vera Cruz, who plagiarized much of Soto's fourth question on Book 7 and shows a misunderstanding of the ratios being presented there; see his *Physica speculatio* (Salamanca, 1562), lib. 7, spec. 5, pp. 141–43. Vera Cruz uses the same expression as Soto for his fourth conclusion on p. 143, which reads: "Velocitas motuum penes causam attenditur penes proportionem proportionum agentium super suas ipsorum resistentias, quae proportio geometrica proportionalitas dicitur." The example he then gives to instantiate this is the following: "Ut si virtus ut .6. moveat resisten-

Since the reception of Soto's ideas among later thinkers, and expecially the Jesuits, is of major interest, next to consider are two different settings where Soto exerted some influence, the first the famous Jesuit college in Rome, the Collegio Romano, and the second Jesuit institutions in Portugal, their *studia* at Evora and Coimbra. The second of these, in particular, may cast some light on the "fifth" rule as it was understood by those who had read Soto's text, if not by Soto himself.

SOTO AND THE COLLEGIO ROMANO

In previous writings I have traced the influence of various aspects of Soto's teachings on professors at the Collegio Romano, particularly those teaching between 1577 and 1591.[87] Here I focus on a remark made by the last of these, Ludovicus Rugerius, who taught the *Physics*, *De caelo*, etc., in the academic year 1590–91. When discussing how the motions of heavy bodies are related in terms of velocity at the beginning, the middle, and the end of their motions, Rugerius ties the answer to this question to Aristotle's rules for the comparison of motions in Book 7; he points out that several of those rules labor under severe difficulties, but for a fuller discussion of the ways in which they might be revised one should read Toletus and Domingo de Soto, among others.[88] The statement is of interest because by this time eight of the nine editions of Soto's text were already in print, with only the second of these, *B*, containing the full parenthetical expression in par. [35] to which reference has been made above. One can only wonder to which edition Rugerius was making reference, but it seems unlikely that edition *B* was what he had in mind.

The recommendation to read Toletus is not especially helpful in this regard. Toletus had been Soto's favorite student at Salamanca, so it is not remarkable that his combined commentary-questionary on the *Physics* should follow closely his teacher's exposition.[89] He too lists the four major

tiam ut quatuor, et virtus ut .3. resistantiam ut duo; quia utrobique est sexquialtera, virtus ut .9. quae constat ex .6. et .3. movebit resistantiam ut .6. quae constat ex .4. et .2. eadem velocitate, quia omnes istae sunt proportiones sexquialterae." That is, $6/4 = 3/2$, and $9/6 = (6+3)/(4+2) = 3/2$; none of these is a geometrical ratio, all being arithmetical and merely a restatement of Aristotle's seventh rule as given in his text 38.

[87] The main discussion is in William A. Wallace, *Galileo and His Sources: The Heritage of the Collegio Romano in Galileo's Science* (Princeton: Princeton University Press, 1984), but the subject is also touched on in several of the publications listed in n. 3 above.

[88] See ibid., p. 189. The Latin reads as follows: "Aliqua ex his theorematis graves patiuntur difficultates, quae tamen apud alios legi possunt. Diligenter Toletus, Sotus, et alii [legendi sunt], apud quos etiam potest videri quo modo Aristoteles hanc doctrinam a motu locali transferat etiam ad alios motus" (Ludovicus Rugerius, *In quatuor libros De caelo et mundo* [1591], MS Bamberg, SB 62–4, fol. 101r–v).

[89] Franciscus Toletus (1533–96) studied under Soto at Salamanca, later taught there himself, and, on becoming a Jesuit, was sent to the Collegio Romano, where he taught logic in

positions on the velocities of motion as they are found in Soto. Only his fourth conclusion, which is accompanied by the marginal notation "Quarto, proportio in velocitate motuum sumitur penes proportionalitatem proportionum virtutum ad suas resistentias," will be considered here. The text reads as follows:

> Quarto vero modo sumitur penes proportionalitatem virtutem [*sic*] ad suas resistentias, quod est dicere ut clare explicemus. Datis duobus motibus, considera in qua proportione virtutes excedant resistentias suas: iterum considera, quae proportio sit inter illas duas proportiones, nam in hac erit unus motus altero velocior. V.g. Sit virtus, ut octo, movens pondera quatuor: sit etiam virtus, ut octo, movens pondera, ut duo, prior virtus est in proportione dupla ad suam resistentiam: posterior vero in quadrupla, puta, octo ad duo. Quia ergo proportio quadrupla est dupla ad duplam, et iste motus erit duplo velocior. Hoc autem modo infallibiliter invenies proportionem velocitatis motuum: si tamen cognoscatur in qua proportione sit una ad aliam: et sic patet solutio argumenti. Non enim illo modo est sumenda proportio velocitatis motuum, puta, penes proportionem excessum [*sic*], sed modo, qui dictus est. Haec de libro septimo.[90]

This is very similar to the text of Paul of Venice, cited above in note 70, which discusses only the doubling and halving of the velocity, force, or resistance, and does not consider the more difficult cases of subalternate and triple ratios considered by Soto. The fact that $2+2$, 2×2, and 2^2 all yield the same result, 4, does not permit one to discriminate between arithmetical operations and exponential or geometrical ratios, and thus to distinguish between the two possible interpretations of Bradwardine's "ratio of ratios" given in note 60 above. And, perforce, there is no mention of a "fifth" rule in either Paul of Venice or Toletus.

Another point to note is that around Rugerius's time at the Collegio Romano there seems to have been an interest in the experimental verification of laws such as Aristotle's. The Jesuit who preceded Rugerius and taught natural philosophy in the academic year 1589–90, Mutius Vitelleschi, remarks on the difficulty of deciding between alternative formulations by means of experiment, though he admits that the results he has seen seem to go contrary to Aristotle. In such a matter, he adds, the data of experience are of crucial importance, and yet there are many things that make him suspect all experiments of this kind. For one, it seems hardly possible that the shapes of the bodies that move, the resistances of the media, and all other things that influence their motions should be equal. For another, one cannot perceive a difference in velocity of this kind unless the motion takes

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1559–60, natural philosophy in 1560–61, and metaphysics in 1561–62, after which he taught theology. He composed many textbooks for use in the order, and later was made a cardinal of the Roman Church.

[90] Franciscus Toletus, *In octo libros Physicorum Aristotelis* (Venice: Apud Iuntas, 1600), lib. 7, tex. 38, q. 5.

place over a very great distance, and over such a distance many things can happen that detract from the certitude of the experiment.[91] The attitude Vitelleschi expresses here may be a sign of a certain diffidence at that time among the Jesuits of the Collegio Romano toward both theoretical and experimental discussions of "laws" of falling bodies.

SOTO AND THE PORTUGUESE JESUITS

A more hopeful line of investigation is the reception of Soto's teaching at Jesuit institutions in Portugal, mainly those at Evora and Coimbra.[92] In fact, in a Coimbra manuscript dating from 1580 I came across a statement very similar to that of Rugerius with which I began the previous section. The context was a discussion of the two questions corresponding to Soto's third and fourth questions on Book 7 of Aristotle's *Physics*, those relating to velocities of motion as ascertained from effect and from cause, respectively. Here the author, pressed by limitations of time in his presentation, refers his readers to the fuller treatments to be found in Domingo de Soto's questions.[93] Following up this lead, I found in other manuscripts materials relating to Soto's fourth question, which I will now discuss—and also ma-

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[91] See Wallace, *Galileo and His Sources* (n. 87), p. 185. The Latin of this passage reads as follows: "Equidem in hac re experientiam nullam omnino certam habeo, et quas habeo, illae potius obstant sententiae Aristotelis. Verum multa sunt qui suspectas mihi faciant omnes experientias de hac re, in qua tamen maxime standum esset experientiae. Primum enim vix videtur posse fieri ut figura corporum qui moventur, resistentia medii, et cetera omnia qui ad motum concurrunt sint omnino paria. Secundo, non potest percipi huiusmodi differentia in velocitate motus nisi motus fiat per magnam distantiam, ut constat etiam experientia. In magna autem distantia multa accidere possunt quae experientiae detrahant aliquid certitudinis. Quare cum haec ita se habeant nihil de hac re statuo, neque enim ad rei materiam necesse est in praesentia" (Mutius Vitelleschi, *In octo libros Physicorum et quatuor De caelo* [1589–90], MS Bamberg, SB 70, fol. 365v). Slightly before this passage Vitelleschi shows his acquaintance with both Bradwardine's treatise on ratios and Jean Taisnier's similar treatise (which was actually a plagiarization of Giovanbattista Benedetti's refutation of Aristotle's laws of free fall, a fact unknown to Vitelleschi), referring his readers to their works as follows: "Legendum est Thomas Bradwardinus in sua tractatione de proportione motuum, et Ioannes Thaisnerus in tractatione de eadem re, qui dicit id demonstrari" (ibid., fol. 365r).

[92] In January 1977, ten years after doing most of my work on the Collegio Romano materials, I spent a week in Portugal, mainly in Lisbon but with a side trip to Coimbra, tracing Jesuit materials relating to Galileo's early notebooks. I copied out by hand my transcription of many manuscripts composed between 1570 and 1587, and reported them in my 1995 article, "Late Sixteenth-Century Portuguese Manuscripts" (see n. 3 above). Unfortunately, my fear of writer's cramp limited what I could copy at the time, and there are more ellipses in my reporting than I would like, but at least it provides clues on which others might later work.

[93] The author is anonymous, but his Latin reads as follows: "Haec duae extremae questiones latius tractantur a Dominico de Soto; ibi possunt videri. Qua de causa praetermittuntur a nobis ac imprimis propter temporis brevitatem" (MS Coimbra BU Res. 2312, fol. 187r). For fuller information on this manuscript and others cited below, and their authors, see my article cited in the previous note.

terials relating to his third question, which I will treat in the following section.

The principal material relating to the velocities of motion as ascertained from their cause is found in two manuscripts, one from Coimbra dating from 1582 and the other from Lisbon dating from 1586.[94] Since these are in almost word-by-word agreement, I give here the reading of the first and supply the few variants of the second in notes. What is interesting about both is that, instead of having four conclusions, as does Soto, they supply five, and thus may throw some light on the "fifth" rule or law to which repeated reference has been made above.

In what follows the Latin text is given first, with paragraphs numbered for future reference, and then the English translation, with technical notes similar to those earlier provided for Soto's question.

Latin text:

> [1] Undenam tamquam ex causa accipienda sit velocitas motus, quaestio secunda....
>
> [2] Prima conclusio.[95] Velocitas motuum non attenditur penes proportionem quam habet una virtus activa ad aliam....
>
> [3] Quinque igitur causas fingere possumus e quarum aliquo[96] sumenda cognoscendaque sit motus[97] velocitas maior, aut minor, aut aequalis. Una earum est proportio virtutum agentium, altera est proportio resistentiarum, tertia est proportio excessuum virtutum agentium supra suas resistentias, quarta est proportio proportionum virtutum agentium ad suas resistentias, ultima est proportio excessuum virtutum agentium supra suas resistentias non simpliciter sed habita ratione quantitatis resistentiarum.
>
> [4] Secunda conclusio.[98] Velocitas motus[99] non attenditur penes proportionem resistentiarum praecise sumptarum....
>
> [5] Tertia conclusio.[100] Velocitas motuum non attenditur simpliciter penes proportionem quam habent excessus virtutum agentium supra suas resistentias....

[94] The first is MS Coimbra, BU Res. 2414, fol. 145v and following, attributed tentatively to Luis de Cerqueira, S.J., by Friedrich Stegmüller in his preliminary study of these manuscripts, and the second is MS Lisbon, BN FG 6283 (no foliation), containing the lectures of Antonio de Castelbranco, S.J. For details, see Friedrich Stegmüller, *Filosofia e teologia nas Universidades de Coimbra e Evora no Seculo XVI* (Coimbra: University of Coimbra, 1959).

[95] conclusio] assertio

[96] aliquo] altera

[97] motus] motuum

[98] conclusio] assertio

[99] motus] motuum

[100] conclusio] assertio

[6] Quarta assertio. Velocitas motuum ex parte causae non est attendenda penes proportionem proportionum virtutum agentium ad suas resistentias....

[7] Quinta assertio, ex iis duobus regulis, quarum prima est si virtutes motivae habuerint aequales proportiones ad suas resistentias tunc motus erunt aequalis velocitatis, ut si virtus ut sex moveat resistentiam ut quatuor, et virtus ut tria resistentiam ut duo, motus erunt aequalis velocitatis....

[8] Secunda regula est si virtutes motivae non habuerint aequales proportiones ad suas resistentias, tunc a virtute maioris proportionis sumatur pars quae habeat tantam proportionem ad suam resistentiam quantam habet virtus minoris proportionis ad suam. Deinde, qualis fuerit proportio excessus partis sumptae supra suam resistentiam ad excessum quem habet tota virtus supra eandem resistentiam, talis erit proportio velocitatis motus facti a virtute maioris proportionis ad velocitatem motus facti a virtute minoris proportionis. Verbi gratia, si virtus ut octo moveat resistentiam ut duo, et virtus ut sex moveat resistentiam ut quatuor, vellisque intelligere qualis sit proportio velocitatis unius motus ad velocitatem alterius facti[101] a virtute ut octo, quae habet maiorem proportionem summe virtutem[102] ut tria, quae habet[103] eandem proportionem ad resistentiam ut duo quam habet virtus ut sex ad resistentiam ut quatuor, cum autem excessus virtutis ut octo supra virtutem etiam[104] ut duo habeat proportionem sextuplam ad excessum quem virtus ut tria habet supra eandem resistentiam, dices virtutem ut octo movere[105] resistentiam ut duo sextuplo velocius quam virtus ut sex moveat resistentiam ut quatuor.

[9] Id quod ita suadebis ex dictis regulis, nam[106] virtus ut tria per primam regulam movet aequa celeritate resistentiam[107] ut duo, atque virtus ut sex resistentiam[108] ut quatuor; sed virtus ut octo movet resistentiam ut duo sextuplo velocius quam virtus ut tria; eo quod excessus virtutis ut octo supra resistentiam ut duo sit sextuplus ad excessum virtutis ut tria supra eandem resistentiam, ergo movet sextuplo velocius quam virtus ut sex movet resistentiam ut quatuor.

Translation:

[1] From what is the velocity of a motion to be ascertained as from a cause, second question....

[2] First conclusion. The velocity of motion is not to be ascertained from the ratio of one active force to another....

[101] alterius facti] alterius
[102] virtutem] virtute
[103] habet] habeat
[104] virtutem etiam] resistentiam
[105] movere] movetur
[106] nam] *om.*
[107] resistentiam] resistentia
[108] resistentiam] resistentia

[3] Five causes can thus be imagined from which one can take and know that the velocity of motions is greater, or less, or equal. One of these is the ratio of the agent forces,[109] another is the ratio of the resistances,[110] a third is the ratio of the excess of agent forces over their resistances,[111] a fourth is the ratio of the ratios of the agent forces to their resistances,[112] and the last is the ratio of the excess of the agent forces over their resistances, not absolutely but taking into account the quantity of the resistances.[113]

[4] Second conclusion. The velocity of motion is not ascertained from the ratio of the resistances taken alone....

[5] Third conclusion. The velocity of motions is not ascertained simply from the ratio that the excesses of the agent forces have over their resistances....

[6] Fourth assertion. The velocity of motions on the part of the cause is not to be ascertained from the ratios of the ratios of the agent forces to their resistances....

[7] Fifth assertion, from the following two rules: The first is that, if motive forces have equal ratios to their resistances, the motions will be of equal velocity. For example, if a force of six moves a resistance of four, and a force of three moves a resistance of two, the motions will be of equal velocity [i.e., $(6 - 4)/4 = (3 - 2)/2$].

[8] The second rule is that, if the motive forces do not have the same ratios to their resistances, then from the force of higher ratio let a part be taken that has the same ratio to its resistance as the force of lesser ratio has to its. Then, whatever the ratio may be that the excess of the part taken over its resistance to the excess that the entire force has over the same resistance, this will be the ratio of the velocity produced by the force of higher ratio to the velocity produced by the force of lesser ratio. For example, if a force of eight moves a resistance of two, and a force of six moves a resistance of four, and you wish to know the ratio of the velocity of one motion to the velocity of another, from the force of eight, which has a greater ratio, take a force equal to three, which has the same proportion to a resistance of two as six has to a resistance of four. Then, since the excess of a force of eight over a resistance also of two

[109] I.e., $V_2/V_1 = F_2/F_1$, basically Soto's formula given in n. 57.

[110] I.e., $V_2/V_1 = R_1/R_2$, Soto's formula in n. 58.

[111] I.e., $V_2/V_1 = (F_2 - R_2)/(F_1 - R_1)$, Soto's formula in n. 59.

[112] This fourth possibility presents more difficulty because of the different ways it may be written, depending on the meaning given to the first "ratio" in the expression "ratio of ratios," as explained in n. 60. If this is a simple arithmetical ratio, then the meaning is $V_2/V_1 = (F_2/R_2)/(F_1/R_1)$—that is, the ratio of the respective ratios of the forces to the resistances. If "ratio" is to be understood in Bradwardine's sense as a geometrical or exponential ratio, however, then the value of V_2/V_1 must be ascertained from the formula $F_2/R_2 = (F_1/R_1)^{V_2/V_1}$, that is, the exponent to which F_1/R_1 must be raised in order to equal F_2/R_2.

[113] I.e., $V_2/V_1 = [(F_2 - R_2)/R_2]/ [(F_1 - R_1)/R_1]$. Edward Grant notes that this formula represents one of the possibilities rejected by Bradwardine himself, although it was later adopted by Giovanni Marliani (d. 1483); see Edward Grant, ed., *A Source Book in Medieval Science* (Cambridge, Mass.: Harvard University Press, 1974), section entitled "Mathematical Representations of Motion," p. 296 n. 15.

would have a sixfold ratio to the excess that a force of three has over the same resistance, you may say that a force of eight moves a resistance of two six times faster than a force of six would move a resistance of four.[114]

[9] And you can argue for the same result from the said rules, for a force of three from the first rule moves a resistance of two with a speed equal to that with which a force of six moves a resistance of four; but a force of eight moves a resistance of two six times faster than a force of three; because the excess of a force of eight over a resistance of two is six times the excess of a force of three over the same resistance. Therefore, it will move [that resistance] six times faster than a force of six moves a resistance of four.[115]

Note in the above excerpts that the first four conclusions or assertions, pars. [2] and [4–6], restate the "laws" of motion approximately as they are found in Soto. The fifth rule is then explained in par. [7]. Here the author does not state this precisely as it appears in par. [3] but rather as a possibility that follows from two additional rules, which he explains in pars. [7–8] and then confirms in par. [9]. The rules seem promising in that they provide examples that can be put to numerical test. On my first reading, in view of the two different meanings that could be given to the first "ratio" in the expression "ratio of ratios," as explained in my notes 60 and 112 above, I tended to think that the author was attempting to carry out calculations found in Soto such as those I explain in my notes 61, 64, and 66 through 68. Now, however, having worked through his numerical examples, I have come to the conclusion that he did not, and, indeed, that when he used the expression "ratio of ratios" he could not have intended it in Bradwardine's sense. Thus he seems of little help in clarifying the referent of Soto's "fifth" law.

A Fifth Law or Not?

What, then, is to be said about Soto's "fifth" law? Did he have one or not? In my view his "fifth" law is basically his fourth, taking "ratio of ratios" in Bradwardine's sense, as he states in par. [22] of the excerpt from his fourth question on Book 7. But the presence of a fifth law in the two Portuguese manuscripts just discussed suggests a possible explanation of the two different numbers occurring in Soto's text. As Edith Sylla has informed me, in

[114] On the reading of the fifth possibility given in the previous note, let F_2 and R_2 take the values of 8 and 2 respectively, and F_1 and R_1 those of 6 and 4 respectively. Now reduce F_2 by 3, so that the formula will be instantiated as $[(8 - 3) - 2]/2$, which is equal to 6/4. Then, for the values $F_2 = 8$, $R_2 = 2$ and $F_1 = 3$, $R_1 = 2$, the ratio V_2/V_1 will be equal to $[(8 - 2)/2]/[(3 - 2)/2]$, that is, 6, so that V_2 will be equal to $6V_1$.

[115] From the calculation at the end of par. [7], $(3 - 2)/2 = (6 - 4)/4$. Similarly, from the calculation at the end of the previous note, $(8 - 2)/2 = 6[(3 - 2)/2]$. Therefore $(8 - 2)/2 = 6[(6 - 4)/4]$, as stated.

presenting the teaching on "ratios" it was not unusual for commentators to discuss five possible "laws" rather than the four entertained by Soto, with the additional "law" being the fifth just analyzed. It is not unlikely that Soto originally planned to include this in his list as a fourth possibility, leaving his fifth and last for his exposition of Bradwardine's version. Then, seeing that this would only confuse matters, he decided to drop the planned fourth possibility and present the Bradwardinian teaching in its place, making what was originally to be the fifth "law" actually his fourth. In cleaning up his manuscript, however, he was not successful in converting all his "fifth"s to "fourth"s, thus presenting us with the textual problem. There is no manuscript evidence to support this conjecture, of course. Yet the fact that none of the editors was concerned with the problem may indicate that the numbering of the "laws" was not a matter of great moment for them or their readership, and that the terms "fourth" and "fifth" were taken simply to refer to Soto's last "law."

Uniformiter Difformis Once Again

Turning now to the counterparts of Soto's third question on Book 7 in the Portuguese materials, one finds Luis de Cerqueira, whose manuscript has just been discussed (MS Coimbra, BU Res. 2414), treating this question explicitly. What is interesting in his account is a passage in which he makes reference to Soto's distinctive teaching—namely, on motion that is "uniformly difform" with respect to time. He does so in a context in which he first explains the term *proportio* and then addresses the question of how the velocity of a motion can be ascertained from its effect. This text is as follows:

> Commentarii in octo Physicorum libros. Annotationes in septimum librum....
>
> Quid et quotuplex sit proportio. Superiori capite docuit Aristoteles quinam motus cum quibus comparari possint. Modo vero tradit nonnulla documenta quibus intelligamus aequales vel inaequales motus quoad velocitatem, quantusque sit excessus velocitatis unius ad velocitatem alterius inter eos qui non sunt aequae celeritatis. Atque primum quidem in motu locali, tum quia motuum sit primus, tum quia in eo proportio haec magis appareat. Docet ergo in motu locali quatuor reperiri, scilicet, movens, mobile, spatium, atque tempus. Nomine autem moventis virtutem seu potentiam motivam, quaecumque ea sit, quae ad motum afferat adiumentum; nomine autem mobilis intelligit resistentiam, quaecumque ea sit, quae motum retardet....
>
> 'Utrum velocitas motus localis sit attendenda ab effectu penes quantitatem spatii, quaestio prima....

> Interdum enim mobile ita difformiter movetur ex parte temporis ut sumpta parte temporis in quo movetur semper in instanti medio tantum superet velocitatem quam habuit vel habebit sub uno instanti terminativo illius temporis quantum superatur a velocitate quam habuit vel habebit sub altero, hic autem motus dicitur uniformiter difformis ex parte temporis convenitque gravibus et levibus cum naturaliter movetur. Semper enim quo magis recedunt a termino a quo eo velocius moventur. Interdum vero mobile non ita uniformiter movetur diciturque talis motus difformiter difformis ex parte temporis, soletque reperiri in motibus progressivo animalium.

Translation:

> Commentaries on the eight books of the Physics. Annotations to the seventh book....
>
> What is a ratio and how many kinds are there? In the previous chapter Aristotle taught that certain motions can be compared with others. Now he treats several teachings that enable us to understand what motions are equal or unequal with regard to velocity, and, among those of unequal speed, how much the velocity of the one exceeds the velocity of the other. And he does so first for local motion, because this type of motion is primary, and because in it ratios are more apparent. He teaches therefore that four factors are found in local motion, namely, a mover, a mobile, space, and time. By a mover he means a motive force or power, whatever it may be, that would offer assistance to the motion. By a mobile he means a resistance, whatever it may be, that would retard the motion....
>
> Whether the velocity of a local motion is to be ascertained as from an effect according to the quantity of the space traversed, first question....
>
> Sometimes the mobile is moved so difformly with respect to time that, taken [any] part of time in which it moves, the velocity it has at the middle instant will exceed the velocity it had or will have at one terminal instant of that time by the same amount as it is exceeded by the velocity it had or will have at another terminal instant. Such a motion is said to be *uniformiter difformis* with respect to time, and it is found in heavy and light bodies when they move naturally, since the more they depart from their starting point the greater is the velocity with which they move. At other times the mobile is not moved uniformly in this way, and then the motion is said to be *difformiter difformis* with respect to time. This is usually found in the progressive motion of animals.

This text is quite unusual in that portions of it are quite similar to the anonymous commentary on the seventh book mentioned at the outset of the previous section, MS Coimbra, BU Res. 2312, whose author referred his readers to Soto's two questions on that book. Not only does that author make clear that the motion of heavy bodies is "uniformly difform with respect to time," but he goes on to explain precisely what this expression means. He writes:

> A motion that is *uniformiter difformis* with respect to time is one where the mobile is so moved that the first part of the motion achieved, namely, that in the first part of the time, surpasses the second [part] in the same ratio as the second surpasses the third. It surpasses, for example, [in this way]: if the stone descends over three squares it will descend *uniformiter difformiter*, for in the second square it will move as much faster than in the first square [as] in the third faster than in the second. And from these considerations, by way of opposition, we can gather what it is for a motion to be *difformiter difformis*, both with respect to the subject and with respect to time.[116]

This same text is found almost verbatim in Luis de Cerqueira's MS Coimbra, BU Res. 2414, of 1582, and MS Lisbon, BN FG 4921, which contains lectures on the *Physics* by the Trinitarian Fr. Marcus de Moura, composed in 1588. From the example given in this text there can be little doubt that all of these authors correctly understood the concept of uniform acceleration as this was later to be understood, and then experimentally verified, by Galileo.

A final question of interest is whether there is any connection between the ways these Portuguese authors conceived variations in velocity ascertained on the part of the cause with those ascertained on the part of the effect. In other words, how precisely did they conceptualize the physical causality involved in the natural motions of heavy and light bodies? Fortunately, this specific question is answered in a passage from commentaries on Aristotle's *De caelo* that is duplicated in manuscripts composed at both Evora and Coimbra. The Latin of the passage is now given from an anonymous exemplar composed in 1582, MS Lisbon, BN FG 4066 (at fol. 113v), which is similar to that contained in MS Coimbra, BU Res. 2414, attributed to Luis de Cerqueira, and likewise composed in 1582.

> Cur gravia et levia naturaliter mota celerius ad finem motus quam in principio ferantur, questio unica.
>
> Circa primam rationem Aristotelis quam sexto capito proposuit quaeritur hoc loco cur ea quae moventur naturaliter motu recto celerior in fine quam in principio motus moveantur, contra vero velocius in principio motus moventur quae violenter mutantur. Denique cur animalia in medio motus celerius moveantur quam in principio ac fine motus.
>
> Quod igitur attinet ad primum, praetermissis multorum responsionibus dicendum est causam huius rei esse quia quemadmodum vis quae in manu projicientis existit cum lapide coniuncta est movendo atque impellendo medio tali

[116] "Motus uniformiter difformis ex parte temporis ille est quando mobile ita movetur ut prima pars motus confecta, scilicet, in parte prima temporis eadem proportione vincat secundam quam secunda tertiam. Vincit, verbi gratia, si descendat lapis per tria quadrantes descendet uniformiter difformiter, nam in secundo quadrante tanto velocius movebitur quam in primo quadrato, in tertio velocius quam in secundo. Ex his per oppositum possumus colligere quisnam sit motus difformiter difformis, tam ex parte subiecti quam ex parte temporis" (fol. 90v).

motu imprimit lapidi talem impulsum quo deinde a manu projicientis separatus movetur, sic etiam gravitas et levitas impellendo rem gravem aut levem ad suum locum naturalem per talem motum imprimit ei impulsum quendam quo interveniente talis motus rei gravis et levis celerior efficitur atque eo magis intenditur talis impetus quo gravia et levia ad sua loca naturalia magis accedunt, quod intelligendum est habita ratione eiusdem termini a quo. Nam si unus idemque lapis modo e medio turris descenderet, postea vero a summitate ipsius multo celerius in fine posterioris motus descenderet quam in fine prioris; quo enim longius est spacium quod percurritur eo maior est impetus a gravitate vel levitate per motum impressum cum continue per talem motum intendatur quousque res ad suum locum naturalem perveniat, quoniam vero impulsus hic efficit in re gravi vel levi similem motu ei qui a gravitate vel levitate proficiscitur. Appellatur idcirco ab Aristotele augmentum gravitatis aut levitatis, ab aliis dicitur gravitas aut levitas accidentaria, quoniam cessante motu amittitur.

Translation:

Why heavy and light bodies when moved naturally are carried more swiftly at the end of the motion than in the beginning, a single question.

Concerning the first argument that Aristotle proposed in the sixth chapter, the inquiry here is why things that are moved naturally in straight-line motion are moved more swiftly at the end of the motion than in the beginning, and, on the contrary, those that are moved violently are moved more swiftly in the beginning. Finally, why is it that animals move more swiftly in the middle of their motion than in the beginning or the end?

With regard to the first, skipping over many replies, it should be said that the reason for this is that, just as the force that exists in the hand of the thrower when it is in contact with the stone and is moving it and impelling the medium impresses on the stone a certain impulse that moves it when separated from the hand of the thrower, so also gravity and levity, impelling a heavy or light body to its natural place, impresses by such motion a certain impulse through whose agency the motion of the heavy or light body is made swifter. And this impetus gets more intense as the heavy and light objects come closer to their natural places, which is to be understood in terms of the relation of each to the *terminus a quo*. For if one and the same stone were now to descend from the middle of a tower and later from its top, it would descend much more swiftly at the end of the later motion than at the end of the earlier. For the longer the space that is traversed, the greater is the impetus impressed by levity and gravity throughout the motion, since it is continually intensified until the body arrives at its natural place. And since this impetus effects in the heavy or light body a motion similar to that which arises in it from gravity and levity, Aristotle referred to it as an increase of gravity and levity; others, however, speak of it as an accidental gravity and levity, since it is lost as soon as the motion stops.

This passage is also contained in a manuscript attributed to Manuel a Lima, S.J., which comes from a course he taught at Evora in 1588, MS Lisbon,

BN FG 2533. It is notable that all three accounts invoke the analogy between *gravitas* and *impetus* pointed out by Soto in his third question on Book 8 of the *Physics*, where he states: "Just as the generator gives the heavy body a natural quality, which is the gravity by which it moves the body all the way to the center, so the projector imparts an impetus to the projectile by which it moves the projectile in a more eminent fashion."[117]

With this I conclude this study of Soto's "laws" of motion and how they were understood in the century in which they were written. Both Soto and the Jesuits who used his work were progressive Aristotelians who did not hesitate to read the concept of impetus into Aristotle's writings. This provided them with a consistent view of causality that enabled them to explain gravitational motion within a Thomistic context, seeing even that type of motion as coming under the influence of the First Unmoved Mover.[118] But as for the motive force that produces such motion, they were wide of the mark—as historians of medieval science would all agree. (The same, of course, could be said of Bradwardine in his more sophisticated attempt in the early fourteenth century.[119]) Thus it would be a mistake to see Soto's "laws" as in a direct line of progress that would lead ultimately to Newton's *Principia.* Soto's more important contribution, in my view, was his *uniformiter difformis* teaching. This continued to be developed by the Jesuits and, as I have argued elsewhere, could well have influenced Galileo in his formulation of the correct law of falling bodies.[120] But that is a different story, one that would take me well beyond the confines of "text and context."

[117] Soto, *Quaestiones* (n. 5), 1555-56: 100vb, "Sicut generans grave tribuit illi naturalem qualitatem, quae est gravitas, qua illud permovet usque ad centrum, sic et proiiciens impingat impetum proiecto, quo ipsum eminus moveat."

[118] On this point, see William A. Wallace, "Aquinas and Newton on the Causality of Nature and of God," in *Philosophy and the God of Abraham*, ed. R. J. Long (Toronto: Pontifical Institute of Mediaeval Studies, 1991), pp. 255–79.

[119] Whether, apart from Soto himself, any of the authors examined in this essay actually understood the expression "ratio of ratios" in Bradwardine's sense is still open to question. Alonso de la Vera Cruz surely did not, as I have explained in n. 86 above. Toletus's numerical example, in contradistinction to Vera Cruz's, checks out satisfactorily, but, as already noted, this is concerned only with the doubling and halving of values, and with these one cannot differentiate between arithmetical and geometrical ratios. The only other Jesuit example I have found is that in the two Portuguese manuscripts analyzed above, and the author of these did not subscribe to Bradwardine's teaching. Whether this would be true of other Jesuit readings is impossible to say on the evidence uncovered thus far. But the cryptic way in which Soto's fourth or fifth "law" is printed in editions *C* through *H* could well have militated against his views being correctly understood by anyone who read them.

* [120] See Wallace, "Early Jesuits" (n. 3); and William A. Wallace, "Domingo de Soto and the Iberian Roots of Galileo's Science," in *Hispanic Philosophy in the Age of Discovery*, ed. Kevin White (Washington, D.C.: Catholic University of America Press, 1997), pp. 113-29..

IV

LATE SIXTEENTH-CENTURY PORTUGUESE MANUSCRIPTS RELATING TO GALILEO'S EARLY NOTEBOOKS

In a number of previous books and articles I have argued the case for a substantial influence of Jesuit teaching materials on Galileo's early notebooks — actually Latin manuscripts he wrote at Pisa roughly between 1589 and 1590, while teaching or preparing to teach at the University of Pisa. The research on which this case is based has been going on for over twenty years, and has taken me to many libraries throughout Europe in search of lecture notes from Jesuit institutions of learning. Most of my evidence derives from lectures given at the Collegio Romano, which I believe to be the proximate source of materials Galileo appropriated. Early in 1977, however, I spent a brief period in Portugal consulting manuscripts from the Collegio Romano that had ended up in the National Library in Lisbon, and also examining notes of lectures given at Evora and Coimbra, the latter at the University Library in that city. From these manuscripts I actually copied out substantial sections on which I have never reported. In this essay I propose to fill that lacuna by giving a few indications of the substantial amount of material in Portuguese manuscripts that pertain to the origins of Galileo's science.[1]

The Latin notebooks to which I refer are written in Galileo's own hand and are now conserved in the Central National Library in Florence: they contain his logical questions (MS Gal. 27), his physical questions (MS Gal. 46), and his early

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[1] My only previous discussion of this material is in my "The Early Jesuits and the Heritage of Domingo de Soto," *History and Technology* 4 (1987), pp. 301-320, reprinted in my *Galileo, the Jesuits, and the Medieval Aristotle*, Collected Studies Series CS346, Hampshire (UK): Variorum, 1991.

treatises on motion (MS Gal. 71). The first of these was appropriated in 1589 from the commentary on Aristotle's *Posterior Analytics* taught by Paulus Vallius, S.J., at the Collegio Romano in 1588 [2]; the second, in 1589-1590 from portions of commentaries on Aristotle's *De caelo* and *De generatione* probably taught by Vallius, although other Jesuit authorship is also possible[3]; and the third contains original drafts by Galileo himself of what is called his *De motu antiquiora*, written in 1590-1591, but for which he used memoranda or notations he had excerpted from various authors, some of whom were Jesuits.[4]

The Portuguese manuscripts I examined in 1977 bear mainly on the second and third of these notebooks. At that time I had not yet discovered the connection between Vallius and MS Gal. 27, which came only in 1980 and has taken up much of my time ever since.[5] A particular point of interest was Galileo's having mentioned the teachings of Domingo de Soto in MS Gal. 46. My previous work on Soto had shown that this Spanish Dominican had, at some time between 1550 and 1555, come to the teaching that the motion of falling bodies is *uniformiter difformis* with respect to time, that is, uniformly accelerated.[6] Galileo is usually credited with making that discovery. What piqued my curiosity was the fact that Soto's favorite student at the University of Salamanca had been Franciscus Toletus. Toletus had become a Jesuit and had been sent to the Collegio Romano to set up its program in philosophy. Was it possible that Soto's teaching on falling motion had been picked up by the Jesuits *via* Toletus and subsequently transmitted to Galileo? In that event my interest would be in the Collegio Romano. Yet the possibility also existed

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2 See my *Galileo's Logical Treatises*. A Translation, with Notes and Comnmentary, of His Appropriated Latin Questions on Aristotle's *Posterior Analytics*. Boston Studies in the Philosophy of Science, 138. Dordrecht-Boston: Kluwer Academic Publishers, 1992.

3 See my *Galileo's Early Notebooks: The Physical Questions*. A Translation from the Latin, with Historical and Paleographical Commentary. Notre Dame: University of Notre Dame Press, 1977.

4 See my *Galileo and His Sources: The Heritage of the Collegio Romano in Galileo's Science*. Princeton: Princeton University Press, 1984.

5 Details are given in *Galileo's Logical Treatises* (note 2) and its companion volume, *Galileo's Logic of Discovery and Proof*. The Background, Content, and Use of His Appropriated Treatises on Aristotle's *Posterior Analytics*. Boston Studies in the Philosophy of Science, 137. Dordrecht-Boston: Kluwer Academic Publishers, 1992.

* 6 "The Enigma of Domingo de Soto: *Uniformiter difformis* and Falling Bodies in Late Medieval Physics," *Isis* 59 (1968), pp. 384-401, reprinted in *Prelude to Galileo: Essays on Medieval and Sixteenth-Century Sources of Galileo's Thought*. Boston Studies in the Philosophy of Science, 62. Dordrecht-Boston: D. Reidel Publishing Co., 1981, pp. 91-109.

that Soto's teaching had influenced other Jesuits on the Iberian peninsula. Entertaining that possibility was what had brought me first to Spain in 1967 and then to Portugal ten years later.

Annotations on the *Physics*

Early in 1977 I had just about finished my translation of, and commentary on, MS Gal. 46, which was to be published later that year as *Galileo's Early Notebooks: The Physical Questions*.[7] These questions, as already remarked, were concerned with materials in Aristotle's *De caelo* and *De generatione*.[8] Now Domingo de Soto had discussed falling bodies not in those treatises, where they are handled by some commentators, but rather in his *Quaestiones* on the seventh book of Aristotle's *Physics*, first published in 1555. Accordingly, it was in commentaries on that work that I first looked for signs of Soto's teaching. I was rewarded immediately in one of the first manuscripts I examined, BN Lisbon FG 4066, anonymous but composed in 1582, which reads as follows:

> Annotationes in septimum librum Physicorum... [fol. 69r]
> Cap. 4. De comparatione motuum et mobilium [fol. 79r]
> Interdum enim mobile ita difformiter movetur ex parte temporis ut sumpta [quavis] parte temporis in qua movetur semper [in] instanti medio tantum superet velocitatem quam habuit vel habebit sub uno instanti terminativo illius temporis quantum superatur a velocitate quam habuit vel habebit sub altero. Hic autem motus dicitur uniformiter difformis ex parte temporis, convenitque gravibus et levibus cum naturaliter moventur. Semper enim quo magis recedunt a termino a quo eo velocius moventur. Interdum enim mobile non ita uniformiter movetur diciturque talis motus difformiter difformis ex parte temporis soletque reperiri in motibus progressivis animalium.[9]

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[7] See note 3.

[8] For a list of the questions in the original Latin, see my "The Dating and Significance* of Galileo's Pisan Manuscripts," in *Nature, Experiment, and the Sciences*. Essays on Galileo and the History of Science in Honour of Stillman Drake. Eds. Trevor Levere and W. R. Shea. Boston Studies in the Philosophy of Science, 120. Dordrecht-Boston: Kluwer Academic Publishers, 1990, p. 16.

[9] *English translation*: Annotations on the seventh book of the *Physics*. Chap. 4. On the comparison of motions and things moved (mobiles). Sometimes the mobile is moved so difformly with respect to time that, taken [any] part of time in which it moves, the velocity it has at the middle instant will exceed the velocity it had or will have at one terminal instant of that time by the same amount as it is exceeded by the velocity it had or will have at another [terminal instant]. Such a motion is said to be *uniformiter difformis* with respect to time, and it is found in heavy and light bodies when they move naturally, since the more they depart

This text is also found in BN Lisbon FG 6283, with the slightly different readings shown above in square brackets; the latter manuscript records the teaching of Antonio de Castelbranco at Evora in 1587. In both cases there is an unmistakable reference to the natural motion of heavy and light bodies as *uniformiter difformis ex parte temporis*, precisely Soto's teaching.

A Coimbra manuscript, like the first manuscript mentioned above also dating from 1582, and attributed tentatively to Luis de Cerqueira, S.J., by Friedrich Stegmüller, is BU Coimbra Cod. Res. 2414.[10] This is unusual in that it incorporates a passage almost identical with that cited above, but in the context of a discussion of the ratios found when comparing motions, which is the same context in which Soto's 1555 passage is to be found. This text first explains the notion of ratio as applied to motions and then inquires whether the velocity of a local motion is to be judged as "from an effect" by the distances the body traverses. It reads:

> Commentarii in octo Physicorum libros.
>
> Annotationes in septimum librum [fol. 132v]...
>
> [Fol. 140r] Quid et quotuplex sit proportio. Superiori capite docuit Aristoteles quinam motus cum quibus comparari possint. Modo vero tradit nonnulla documenta quibus intelligamus aequales vel inaequales motus quoad velocitatem, quantusque sit excessus velocitatis unius ad velocitatem alterius inter eos qui non sunt aequae celeritatis. Atque primum quidem in motu locali, tum quia motuum sit primus, tum quia in eo proportio haec magis appareat. Docet ergo in motu locali quatuor reperiri, scilicet, movens, mobile, spatium, atque tempus. Nomine autem moventis virtutem seu potentiam motivam, quaecumque ea sit, quae ad motum afferat adiumentum; nomine autem mobilis intelligit resistentiam, quaecumque ea sit, quae motum retardet....
>
> Utrum velocitas motus localis sit attendenda ab effectu penes quantitatem spatii, quaestio prima.
>
> ...Interdum enim mobile ita difformiter movetur ex parte temporis ut sumpta parte temporis in quo movetur semper in instanti medio tantum superet velocitatem quam habuit vel habebit sub uno instanti terminativo illius temporis quantum superatur a velocitate quam habuit vel habebit sub altero, hic autem motus dicitur uniformiter difformis ex parte temporis convenitque gravibus et levibus cum naturaliter movetur. Semper enim quo magis recedunt a termino a quo eo velocius moventur. Interdum vero mobile non ita uniformiter movetur diciturque talis motus difformiter difformis ex parte temporis, soletque reperiri in motibus progressivo animalium.[11]

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from their starting point the greater is the velocity with which they move. At other times the mobile is not moved uniformly in this way, and then the motion is said to be *difformiter difformis* with respect to time. This is usually found in the progressive motion of animals.

[10] See Stegmüller's *Filosofia e Teologia nas Universidades de Coimbra e Evora no Seculo XVI*, Coimbra: University of Coimbra, 1959, p. 255.

[11] *English translation:* Commentaries on the eight books of the *Physics*. Annotations to the seventh book.

The question of how the velocity of motion is to be judged as "from an effect" is posed in the seventh book of Soto's *Quaestiones* on the *Physics*, where it is followed by a related question, namely, how the velocity of motion is to be judged as "from a cause." This was common practise in treatises composed at the University of Paris, where Soto had studied, in the early decades of the sixteenth century. A search for that question in the Coimbra manuscript already mentioned, BU Coimbra Cod. Res. 2414, found it there also, accompanied by a rather full discussion. This reads as follows:

> Undenam tamquam ex causa accipienda sit velocitas motus, quaestio secunda....
>
> Prima conclusio [assertio]. Velocitas motuum non attenditur penes proportionem quam habet una virtus activa ad aliam....
>
> Quinque igitur causas fingere possumus e quarum aliqua [altera] sumenda cognoscendaque sit motus [motuum] velocitas maior. aut minor. aut aequalis. Una earum est proportio virtutum agentium. altera est proportio resistentiarum, tertia est proportio excessuum virtutum agentium supra suas resistentias. quarta est proportio proportionum virtutum agentium ad suas resistentias, ultima est proportio excessuum virtutum agentium supra suas resistentias non simpliciter sed habita ratione quantitatis resistentiarum.[12]
>
> Secunda conclusio [assertio]. Velocitas motus [motuum] non attenditur penes proportionem resistentiarum praecise sumptarum....
>
> Tertia conclusio [assertio]. Velocitas motuum non attenditur simpliciter penes proportionem quam habent excessus virtutum agentium supra suas resistentas....
>
> Quarta assertio. Velocitas motuum ex parte causae non est attendenda penes proportionem proportionum virtutum agentium ad suas resistentias...
>
> Quinta assertio. ex iis duobus regulis. quarum prima est si virtutes motivae habuerint aequales proportiones ad suas resistentias tunc motus erunt aequalis velocitatis.

What is a ratio and how many kinds are there? In the previous chapter Aristotle taught that certain motions can be compared with others. Now he treats several teachings that enable us to understand what motions are equal or unequal with regard to velocity, and, among those of unequal speed. how much the velocity of the one exceeds the velocity of the other. And he does so first for local motion, because this type of motion is primary, and because in it ratios are more apparent. He teaches therefore that four factors are found in local motion, namely. a mover. a mobile, space. and time. By a mover he means a motive force or power, whatever it may be, that would offer assistance to the motion. By a mobile he means a resistance, whatever it may be. that would retard the motion...

Whether the velocity of a local motion is to be ascertained as from an effect according to the quantity of the space traversed, first question.

Sometimes the mobile... [same translation as in note 9].

12 These ratios can be expressed mathematically by the following formulas, assuming that V is the velocity, F the motive force, and R the resistance: (1) $V_1/V_2 = F_1/F_2$; (2) $V_1/V_2 = R_2/R_1$; (3) $V_1/V_2 = (F_1-R_1)/(F_2-R_2)$; (4) $V_1/V_2 = (F_1/R_1)/(F_2/R_2)$; and (5) $V_1/V_2 = [(F_1-R_1)/R_1]/[(F_2-R_2)/R_2]$.

ut si virtus ut sex moveat resistentiam ut quatuor, et virtus ut tria resistentiam ut duo, motus erunt aequalis velocitatis...

Secunda regula est si virtutes motivae non habuerint aequales proportiones ad suas resistentias, tunc a virtute maioris proportionis sumatur pars quae habeat tantam proportionem ad suam resistentiam quantam habet virtus minoris proportionis ad suam. Deinde, qualis fuerit proportio excessus partis sumptae supra suam resistentiam ad excessum quem habet tota virtus supra eandem resistentiam, talis erit proportio velocitatis motus facti a virtute maioris proportionis ad velocitatem motus facti a virtute minoris proportionis. Verbi gratia, si virtus ut octo moveat resistentiam ut duo, et virtus ut sex moveat resistentiam ut quatuor, vellisque intelligere qualis sit proportio velocitatis unius motus ad velocitatem facti [alterius] a virtute ut octo, quae habet maiorem proportionem summe virtutum ut tria, quae habet [habeat] eandem proportionem ad resistentiam ut duo quam habet virtus ut sex ad resistentiam ut quatuor, cum autem excessus virtutis ut octo supra virtutem [resistentiam] etiam ut duo habeat proportionem sextuplam ad excessum quem virtus ut tria habet supra eandem resistentiam, dices virtutem ut octo movere [movet] resistentiam ut duo sextuplo velocius quam virtus ut sex moveat resistentiam ut quatuor.

Id quod ita suadebis ex dictis regulis, nam virtus ut tria per primam regulam movet aequa celeritate resistentiam ut duo, atque virtus ut sex resistentiam ut quatuor; sed virtus ut octo movet resistentiam ut duo sextuplo velocius quam virtus ut tria; eo quod excessus virtutis ut octo supra resistentiam ut duo sit sextuplus ad excessum virtutis ut tria supra eandem resistentiam, ergo movet sextuplo velocius quam virtus ut sex movet resistentiam ut quatuor.[13]

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[13] *English translation:* From what is the velocity of a motion to be ascertained as from a cause, second question...

First conclusion [assertion]. The velocity of motion is not to be ascertained from the ratio of one active force to another...

Four causes can be imagined from which one can take and know that the velocity of motions is greater, or less, or equal. One of these is the ratio of the agent forces, another is the ratio of the resistances, a third is the ratio of the excess of agent forces over their resistances, a fourth is the ratio of the ratios of the agent forces to their resistances, and the last is the ratio of the excess of the agent forces over their resistances, not absolutely but taking into account the quantity of the resistances.[12]

Second conclusion [assertion]. The velocity of motion [motions] is not ascertained from the ratio of the resistances taken alone....

Third conclusion [assertion]. The velocity of motions is not ascertained simply from the ratio that the excesses of the agent forces have over their resistances....

Fourth assertion. The velocity of motions on the part of the cause is not to be ascertained from the ratios of the agent forces to their resistances....

Fifth assertion [comes] from the following two rules: The first is that, if motive forces have equal ratios to their resistances, the motions will be of equal velocity. For example, if a force of six moves a resistance of four, and a force of three moves a resistance of two, the motions will be of equal velocity.

The second rule is that, if the motive forces do not have the same ratios to their resistances, then from the force of higher ratio let a part be taken that has the same ratio to its resistance as the force of lesser ratio to its. Then, whatever the ratio may be that the excess of the part taken over its resistance to the excess that the entire force has over the same resistance, this

As in the previous passage cited from BU Lisbon FG 4066, the foregoing passage was also appropriated by Antonio de Castelbranco in 1587 and appears in his BU Lisbon FG 6283, again with alternative readings as indicated above in square brackets. It is likewise found in another Coimbra manuscript, though in a more primitive form: this is BU Coimbra Cod. Res. 2313, which records a commentary on the *Physics* composed by Fr. Juan Gomez de Braga (Ioannis Gomesii Bracharensis) in 1570. This could well be one of the sources on which the accounts described above depend.

Yet another manuscript which supplies additional details is BN Lisbon FG 4921, containing the *Annotationes in octo libros physicorum Aristotelis* of Fr. Marcus de Moura, O.S.T., composed in 1588. Like the foregoing, the passage from Moura is contained in his questions on the seventh book and is situated in a similar context. The passage is brief and reads as follows:

> Quid et quotuplex sit proportio
>
> Sitne attenda velocitas motus localis penes quantitatem spacii quod a mobili conficitur?
>
> ...Motus uniformiter difformis ex parte temporis ille est quando mobile ita movetur ut prima pars motus confecta, scilicet, in parte prima temporis eadem proportione vincat secundam quam secunda tertiam. Vincit, verbi gratia, si descendat lapis per tria quadrantes descendet uniformiter difformiter, nam in secundo quadrante tanto velocius movebitur quam in primo quadrato, in tertio velocius quam in secundo. Ex his per oppositum possumus colligere quisnam sit motus difformiter difformis, tam ex parte subiecti quam ex parte temporis.[14]

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will be the ratio of the velocity produced by the force of higher ratio to the velocity produced by the force of lesser ratio. For example, if a force of eight moves a resistance of two, and a force of six moves a resistance of four, you wish to know what the ratio of the velocity of one motion to the velocity [of another?] by a force of eight, which has a greater ratio of summed forces [equal to] three, which would have the same proportion to a resistance of two as six has to a resistance of four. But since the excess of a force of eight over a resistance also of two would have a sixfold ratio to the excess that a force of three has over the same resistance, you should say that a force of eight moves a resistance of two six times faster than a force of six would move a resistance of four.

And you can obtain the same result from the said rules, for a force of three from the first rule moves a resistance of two with a speed equal to that with which a force of six [moves] a resistance of four; but a force of eight moves a resistance of two six times faster than a force of three; but from the fact that the excess of a force of eight over a resistance of two is six times the excess of a force of three over the same resistance, it follows that it will move six times faster than a force of six moves a resistance of four.

[14] *English translation:* What is a ratio and how many kinds are there? Is the velocity of a local motion to be ascertained from the amount of space that is covered by the mobile?

...A motion that is *uniformiter difformis* with respect to time is one where the mobile is so moved that the first part of the motion achieved, namely, that in the first part of the time,

From the example given here there can be no doubt that Moura had correctly understood the concept of uniform acceleration as this was later to be understood, and then experimentally verified, by Galileo.

These texts from commentaries on the seventh book of the *Physics*, when studied in relation to Soto's passages in his *Quaestiones* on the same book, give clear indication that the Spanish Dominican is the ultimate source from which they derive. A further confirmation is provided in yet another Coimbra manuscript, BU Coimbra Cod. Res. 2312, written in 1580. The author of these *Annotationes in octo physicorum libros Aristotelis* is anonymous, but in commenting on the seventh book he replies to the same two questions relating to the velocity of local motion as the authors already cited. The passage is important not so much for his answers to these questions as it is for the remark he makes after providing them. Here he makes explicit reference to the teaching of Domingo de Soto:

> In septimum librum physicorum...
> [Fol. 180r] Cap. quintum. Superiori capite docuit Aristoteles...
> [Fol. 181r] Utrum velocitas motus localis a quantitate spatii quod pertransitur tamquam ab effectu sit colligenda, quaestio prima ...
> [Fol. 183v] Undenam tamquam ex causa accipienda sit velocitas motus, quaestio secunda...
> [Fol. 187r] Haec duae extremae questiones latius tractantur a Dominico de Soto; ibi possunt videri. Qua de causa praetermittuntur a nobis ac imprimis propter temporis brevitatem.[15]

The materials on fols. 180r and 181r are the same as those in BU Coimbra Cod. Res. 2414, cited above, and that on fol. 183v is the same as that in BN Lisbon FG 4921, also cited above. The additional statements on fol. 187r would seem to leave little doubt that Soto was the source from which these Portuguese professors obtained their teaching about uniform acceleration in falling motion.

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surpasses the second [part] in the same ratio as the second surpasses the third. It surpasses, for example, [in this way]: if the stone descends over three squares it will descend *uniformiter difformiter*, for in the second square it will move with greater velocity than in the first square, and in the third with greater velocity than in the second. And from these considerations, by way of opposition, we can gather what it is for a motion to be *difformiter diformis*, both with respect to the subject and with respect to time.

[15] The citation on fol. 187r translates as follows: "These last two questions are treated more fully by Domingo de Soto and can be studied there. For this reason, and especially because of limitations of time, we will pass over them quickly."

Annotations on *De Caelo* and *De Generatione*

A different passage from manuscripts composed at Evora and Coimbra is to be found in commentaries on Aristotle's *De caelo*. This too provides evidence of derivation from Soto's *Quaestions*, though in a different way. The problem, as heretofore, was why bodies speed up as they fall naturally, in the case of heavy bodies, and rise naturally, in the case of light bodies. An anonymous exemplar, composed in 1582, is found in the Lisbon manuscript cited several times above, BN Lisbon FG 4066. The passage reads as follows:

> [Fol. 113v] Cur gravia et levia naturaliter mota celerius ad finem motus quam in principio ferantur, questio unica.
>
> Circa primam rationem Aristotelis quam sexto capito proposuit quaeritur [quaeri solet] hoc loco cur ea quae moventur naturaliter motu recto celerior [celerius] in fine quam in principio motus moveantur, contra vero velocius in principio motus moventur quae violenter mutantur. Denique cur animalia in medio motus celerius moveantur quam in principio ac fine motus.
>
> Quod igitur attinet ad primum, praetermissis multorum responsionibus dicendum est causam huius rei esse quia quemadmodum vis quae in manu projicientis existit cum lapide coniuncta est movendo atque impellendo medio [medium] tali motu imprimit lapidi talem [tali] impulsum [+ quendam] quo deinde a manu projicientis separatus movetur, sic etiam gravitas et levitas impellendo rem gravem aut levem ad suum locum naturalem per talem motum imprimit ei impulsum quendam quo interveniente talis motus rei gravis et levis celerior efficitur atque eo magis intenditur talis impetus quo gravia et levia ad sua loca naturalia magis accedunt, quod intelligendum est habita ratione eiusdem termini a quo. Nam si unus idemque lapis modo e medio turris descenderet, postea vero a summitate ipsius multo celerius in fine posterioris motus descenderet quam in fine prioris; quo enim longius est spacium quod percurritur eo maior est impetus a gravitate vel levitate [levitate vel gravitate] per motum impressum cum continue per talem motum intendatur quousque res ad suum locum naturalem perveniat [pervenit], quoniam vero impulsus hic efficit in re gravi vel [aut] levi similem motu ei qui a gravitate vel levitate proficiscitur. Appellatur idcirco ab Aristotele augmentum gravitatis aut levitatis, ab aliis dicitur gravitas aut levitas accidentaria [accidentarie], quoniam cessante motu amittitur.[16]

[16] *English translation:* Why heavy and light bodies when moved naturally are carried more swifly at the end of the motion than in the beginning, a single question.

Concerning the first argument that Aristotle proposed in the sixth chapter, the inquiry here is why things that are moved naturally in straight-line motion are moved more swiftly at the end of the motion than in the beginning, and, on the contrary, those that are moved violently are moved more swiftly in the beginning. Finally, why is it that animals move more swiftly in the middle of their motion than in the beginning or the end.

This same passage is found in a Coimbra manuscript, BU Coimbra Cod. Res. 2414, also dating from 1582, with the variants noted above in square brackets. It is likewise found in BN Lisbon FG 2533, which records the lectures given by Manuel a Lima in 1588.[17]

Generally the Portuguese expositions of Aristotle's *De caelo* are not as extensive as those found in the Collegio Romano notes dating from roughly the same period. Since Manuel a Lima's treatment is representative, we reproduce here the outline of his lectures on the books of *De caelo et mundo*:

> BN Lisbon FG 2533. Annotationes in universam Aristotelis philosophiam naturalem traditae a sapientissimo praeceptore meo Emmanuele a Lima, anno Domini 1588.
>
> **Annotationes in libros de caelo...**
> **[In primum librum:]**
> De subiecto [A]
> Obiectiones adversus Aristotelem...
> Utrum motus circularis sit caelo naturalis
> Utrum caelum constet materia [K]
> Utrum materia caeli differat [specie] a materia horum [rerum] generabilium [K]
> Quomodo potentiae activae terminentur [T]
> Quomodo potentiae passivae terminentur [T]

With regard to the first, skipping over many replies, it should be said that the reason for this is that, just as the force that exists in the hand of the thrower when it is in contact with the stone and is moving it and impelling the medium impresses on the stone a certain impulse that moves it when separated from the hand of the thrower, so also gravity and levity, impelling the heavy and light body to its natural place, impresses by such motion a certain impulse through whose agency the motion of the heavy and light body is made swifter. And this impetus gets more intense as the heavy and light objects come closer to their natural places, which is to be understood in terms of the relation of each to the *terminus a quo*. For if one and the same stone were now to descend from the middle of a tower and later from its top, it would descend much more swiftly at the end of the later motion than at the end of the earlier. For the longer the space that is traversed the greater is the impetus impressed by levity and gravity throughout the motion, since it is continually intensified until the body arrives at its natural place. And since this impetus effects in the heavy or light body a motion similar to that which arises in it from gravity and levity, Aristotle referred to it as an increase of gravity and levity; others, however, speak of it as an accidental gravity and levity, since it is lost as soon as the motion stops.

[17] It is noteworthy that all three accounts invoke the analogy between *gravitas* and *impetus* pointed out by Domingo de Soto in his *Quaestiones* on the *Physics*, Lib. 8, q. 3: "Sicut generans grave tribuit illi naturalem qualitatem, quae est gravitas, qua illud permovet usque ad centrum, sic et proiiciens impingat impetum proiecto, quo ipsum eminus moveat." ("Just as the generator gives the heavy body a natural quality, which is the gravity by which it moves the body all the way to the center, so the projector imparts an impetus to the projectile by which it moves the projectile in a more eminent fashion.")

In secundum librum:
Utrum caeli sint animati [L]
Utrum in caelo sint ex natura rei sex positionum differentiae
Cur gravia et levia naturaliter mota celerius in fine motus quam in principio ferantur
Utrum astra omnia a sole lumen accipiant
In quartum librum:
Utrum inter elementa dantur unum [quoddam] simpliciter leve, alterum [aliud] simpliciter grave, caetera vero sint gravia et levia per comparationem
Utrum elementa in suis locis naturalibus gravitent et levitent
Impositus fuit finis his libris de caelo 2ª die mensis Maii anno Domini 1589[18]

The same outline may be found in BU Coimbra Cod. Res. 4066, with the changes enclosed above in square brackets.

Very similar to these lectures on the *De caelo* are those of the Trinitarian Marcus de Moura who lectured at Lisbon, also in 1588-1589. The codex in which it is found, BN Lisbon FG 4921, contains Moura's exposition of the two books of Aristotle's *De generatione*. Like the annotations on the *De caelo*, this is fairly brief; its contents read as follows:

BN Lisbon FG 4921 [**Annotationes in libros *De generatione et corruptione* a fr. Marco de Moura**]
In primo libro:
Utrum in eodem composito dentur multae formae substantiales
Subiecteturne accidentia corporalia in materia prima an in toto composito
Utrum in corruptione fiat resolutio usque ad materiam primam [R]
Quid sit augmentatio
Quomodo fiat augmentatio viventis
Utrum in accretione maneat idem numero subiectum
Utrum augmentation sit motus continuus
Utrum in rarefactione acquiratur nova quantitas
Totum tripliciter sumi potest
Utrum idem possit agere in seipsum et simile in simile
Utrum omne agens communicans cum patiente in materia eiusdem rationis agendo repatiatur
Possitne esse mixtio
Utrum elementa maneant formaliter in mixtis
In secundo libro:
Quaenam sint quatuor primae qualitates [V]
Utrum contraria possint esse simul in eadem re
Sintne recte collecta quattuor elementa [U]

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18 The letters enclosed in square brackets indicate similar questions in Galileo's notes on the *De caelo* translated in *Galileo's Early Notebooks* (note 3).

Argumentatur contra quasdam conclusiones Aristotelis
Utrum detur mixtum temperatum quoad elementorum qualitates
Utrum idem numero per naturam reddire possit
Utrum idem numero divina virtute possit reparari[19]

Those interested in the much more extensive treatments of the heavens and the elements at the Collegio Romano when Manuel a Lima and Marcus de Moura were teaching at Evora and Lisbon will find a listing of all the questions on the *De caelo* and *De generatione* of Antonius Menu, S.J. (1578), Mutius Vitelleschi, S.J. (1589-1590), and Ludovicus Rugerius, S.J. (1590-1591), who covered this material in the years given in parentheses, in an essay of mine published in 1990.[20] Also indicated in that essay are the contents of Galileo's MS Gal. 46 and of the course offered by Paulus Vallius, S.J., on *De elementis* some time between 1585 and 1589. In addition there is a tabulation of textual parallels between the paragraphs of Galileo's exposition and corresponding paragraphs in the teaching notes of all of the above Roman professors.[21] These data reveal a continuity of teaching on these matters in Rome from 1578 to 1591, which mirrors to some extent that found at Evora and Coimbra during the same period.

More Collegio Romano Materials

At the outset I remarked that I had gone to Portugal to consult additional teaching notes from Rome that had ended up in the National Library of Lisbon. The notes to which I was referring were those of Robertus Jones, S.J., who lectured on the *De caelo* in 1593, and Stephanus del Bufalo, S.J., who lectured on the *De caelo* and *De generatione* in 1596-1597. So as to complete the listing of textual parallels with Galileo's physical questions in Jesuit teaching notes, I have transcribed below the titles of the questions in the notes of Jones and Del Bufalo. Since Jones's annotations are brief, in his case I also add some of his replies. Galileo's questions are twenty-five in number, and these are identified by capital letters from A to Y in my English translation of them, *Galileo's Early Notebooks: The Physical Questions*. Where one of these letters appears after the title of a question below, this is an indication that material similar to Galileo's is also found in the lectures of Jones and Del Bufalo.

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19 Cf. the previous note, substituting *De generatione* for *De caelo*.
* 20 "The Dating and Significance of Galileo's Pisan Manuscripts" (note 8), pp. 16-27.
21 Ibid., pp. 16, 21-22, 28-31.

BN Lisbon FG 2066 Robertus Jones, S.I. Romae 1593
Disputatio quinta *Physicae*
De universo, vel de coelo et mundo

Quaestio prima. Quid de facto dicendum sit circa originem mundi [D]. Nos vero una cum theologis et philosophis Christianis asserimus mundum non fuisse factum ab aeterno, et est de fide, verum ut haec veritas eluceat dirimenda sunt argumenta Aristotelis, Procli, Simplicii, et Averrois, cum putent hoc dogma demonstrari.

Quaestio secunda. Quid de possibili dicendum sit circa originem mundi [F]. Conclusio unica. Mundus non solum secundum entia incorruptibilia et corruptibilia praeservata a corruptione et mutatione, sed etiam secundum entia curruptibilia, successiva, et mutationis obnoxi; consequenter mundus absolute potuit esse ab aeterna. Haec conclusio ut constat est communiter philosophorum et theologorum sententia, praesertim St. Thomas et Thomistae.

Quaestio tertia. An coelum sit ex elementis compositum [I]. Dico primo celum non est ullum ex quatuor elementis.... Dico secundo coelum non est aliquid mixtum ex elementis....

Quaestio quarta. An coelum constet ex materia et forma [K]. Conclusio unica. Coelum constat ex materia et forma physica informante....

Quaestio quinta. An materia coeli sit eiusdem rationis cum materia inferiorum [K]. Dico primo materia coeli non est eiusdem rationis cum materia inferiorum, consequenter non est eadem numero cum illa.... Dico secundo quicquid sit de numero coelorum, item quicquid sit de omocentricis, excentricis, et epiciclium, de quibus omnibus standum est iudicio astronomorum, probabilius est tot esse materias diversas specie et numero inter se in corporibus coelestibus quot sunt coeli totales distincti numero....

Quaestio sexta. An coelum sit incorruptibile [J]. Conclusio unica. Coelum natura sua est incorruptibile, immo in die iudicii non est immutandum substantialiter....

Quaestio septima. An coelum sit animatum ulla anima [L]. Dico primo coelum non est animatum ulla anima sensitiva vel vegetativa. Dico secundo nec ex Aristotele nec ex veritate coelum est informatum aliqua anima rationali aut etiam ab aliqua intelligentia quae det esse formaliter orbi ita ut sit forma informans respectu illius....

Quaestio octava. A quo moveatur coelum, et an motus illius sit naturalis.

Quaestio nona. An et qua ratione coelum agat in hac inferiora.

Quaestio decima. De reliquis coeli accidentibus.

When one compares this list of questions with the lists of questions of Menu, Vitelleschi, and Rugerius referred to above, one sees that Jones does not follow the usual pattern. He is extremely brief and has no treatment whatever of the elements, their gravity and levity, as was found in most of the Roman commentaries on the *De caelo*.

Stephanus del Bufalo, on the other hand, offered a complete treatment of both the *De caelo* and the *De generatione*. Here we transcribe only his lists of questions and constituent articles for these commentaries. Again the questions that contain passages similar to those found in Galileo's MS Gal. 46 are

indicated by the letters used to identify Galileo's questions in my *Galileo's Early Notebooks: The Physical Questions.*

BN Lisbon FG 1892. A patre Stephano del Bufalo Societatis Iesu Romae Anno 1596.

In quatuor libros caelorum.

Disputatio prima. De mundi origine respondens iis qui octavo Physicorum et primo et secundo *De caelo* ait Aristoteles.

Quaestio prima. Quomodo mundus creatus est.

Art. 1. Utrum de facto mundus creatus fuit ab aeterno [D].

Art. 2. Utrum de facto sint plures mundi [E].

Art. ult. Utrum necessario debeat esse unus, et quam unitatem habeat hic mundus [E].

Quaestio secunda. An lumine naturali possit cognosci mundum habere esse in tempore per creationem, et quid de hac re Aristoteles.

Art. 1. Utrum possit cognosci lumine naturali universum esse creatum a Deo in tempore.

Art. 2. Quid senserit de creatione Aristoteles caeterique Antiqui [C].

Art. 3. Utrum secundum Aristotelem Deus sit causa efficiens rerum omnium.

Art. ult. Utrum in sententia Aristotelis Deus sit causa efficiens mundi ex necessitate naturae et non libera.

Quaestio ultima. Utrum mundus potuerit esse ab aeterno.

Art. unicus. Utrum aliquod ens creatum potuerit esse ab aeterno [F].

Disputatio secunda. De caelo.

Quaestio prima. Quae sit caeli natura.

Art. 1. Utrum caelum sit corpus mixtum [I].

Art. 2. Utrum caelum sit corpus simplex vel unum ex <quatuor elementis> [I].

Art. 3. Quomodo efficaciter probatur ab Aristotele caelum esse corpus simplex diversum natura ab elementis.

Art. 4. Utrum caelum sit corpus simplex incorruptibile [J].

Art. 5. Quomodo caelum sit corpus simplex num compositum ex materia et forma [K].

Art. 6. Utrum materia caeli sit eiusdem rationis cum hac nostra, et sitne corruptibile [K].

Art. ult. Utrum caelum sit compositum ex forma quae sit anima [L].

Quaestio secunda. Circa caeli accidentia.

Art. 1. Quaenam accidentia habeat caelum.

Art. 2. Utrum haec accidentia quae sunt in caelo sunt eiusdem rationis cum his inferioribus.

Art. 3. Utrum caelum moveatur ab intelligentia [L].

Art. 4. Utrum motus caeli sit naturalis et quomodo.

Art. ult. Utrum caeli differant specie inter se an non.

Quaestio ultima. Quam efficientiam habeant caeli in haec inferioria.

Art. unicus. Utrum caelum sit causa ita universalis ut immediate per se concurrat ad omnes actiones corporum.

In duos libros Aristotelis *De generatione et corruptione.*
Disputatio prima. De essentia generationis eiusque causis.
Prima pars. De generatione proprie dicta eiusque causis.
Quaestio prima. Quid sit generatio proprie dicta.
Art. 1. Quae sit definitio huius generationis.
Art. 2. Utrum generatio sit mutatio tota in totum ens.
Art. 3. Utrum in generatio praedicta bene ponantur illa verba nollu sensibus ut subiecto eodem."
Art. 4. Utrum ut sit vera et essentialis generatio debeat produci forma vi generationis.
Art. ult. Utrum generatio sit aliqua entitate distincta a termino, et utrum essentialiter sit successiva et in quo differat a corruptione.
Quaestio secunda. De causis generationis et corruptionis.
Art. 1. Utrum secundae [causae], i.e. creaturae, habeant suas naturales actiones, an solum causa prima agat ad praesentiam illarum.
Art. 2. Utrum Deus ad singulares agentium creationis actiones concurrat immediate sua actione.
Art. 3. Utrum causae secundae agant immediate an mediantibus accidentibus.
Art. ult. Quomodo accidens sit instrumentum substantiae et qua ratione substantia concurrat ad actionem accidentium tamquam causa principalis.
Secunda pars. De natura alterationis
Quaestio prima. De quidditate alterationis
Art. 1. De qua alteratione in praesenti agendum sit.
Art. 2. Quae sit definitio alterationis et quo primo a generatione distinguatur [M].
Quaestio secunda. Quomodo acquirantur qualitates per alterationem
Art. 1. In quo sit status totius quaestionis.
Art. 2. Quid sit qualitatem intendi in subiecto sive quid sit qualitatem habere plures gradus in subiecto [N,O]
Quaestio tertia. De successione et continuitate alterationis
Art. 1. Utrum alteratio sit continua successio.
Art. 2. Utrum gradus unius qualitatis sit eiusdem rationis et quomodo [O].
Art. ult. Utrum alteratio sit continua extensive in subiecto.
Quaestio ultima. De contrarietate qualitatum
Art. unicus. Utrum contrariae qualitates possint esse simul in eodem secundum aliquos gradus et quomodo.
Disputatio secunda. De iis quae praerequiruntur ad mixtionem
Pars prima. De actione et passione
Quaestio prima. Utrum actio et passio sint solum inter ea quae habent subiectum commune, et sunt dissimilia secundum formam.
Art. 1. Utrum et quomodo simile agat in simile.
Art. 2. Utrum idem possit agere in seipso.
Quaestio ultima. Utrum omne agens physicum in agendo repatiatur.
Art. 1. Quid sit unum resistere alii [X].
Art. 2. Quaenam primae qualitates sint magis activae et quaemagis resistivae et quaenam sit maioris [!] activa quam resistiva [W].
Art. 3. Quaenam conferant ad maiorem actionem agentis.

Art. 2. Utrum hae qualitates motivae sint congenitae iis corporibus an aliunde oriuntur.

Art. 3. Utrum praedicate qualitates motivae quibus elementa moventur differant solum secundum magis et minus an vero essentialiter.

As can be seen from this lengthy list of questions, Del Bufalo's treatment of the materials in the *De caelo* and the *De generatione et corruptione* is as extensive as those of Menu, Vitelleschi, and Rugerius, his predecessors at the Collegio Romano. Obviously they are far more detailed than the materials covered by Manuel de Lima and Marco de Moura in their expositions of these books.

A curious fact is that some parts of Del Bufalo's exposition, which dates from 1596-1597, show closer agreement than do those of Menu, Vallius, Vitelleschi, and Rugerius with Galileo's text, a text that from all other indications was composed in 1589-1590. Some sample texts illustrating this are given in *Galileo and His Sources*.[22] The anomaly is difficult to explain, but one possibility is that, in preparing his lectures, Del Bufalo made use of the same version of the previous notes as was available to Galileo (presumably Vallius's). This would explain the similarities with this late source and would not require one to maintain that MS Gal. 46 was not composed until 1597 or thereafter.

Additional Reflections

A noteworthy feature of MS Gal. 46 is that it contains two questions, one on the number of the heavenly orbs [G] and the other on their order [H], which seem to have been appropriated directly from Christopher Clavius's commentary on the *Sphaera* of Sacrobosco. None of the Portuguese notes I have examined presents material of this type, nor is it found in the expositions of the *De caelo* of Menu, Vitelleschi, and Rugerius. Many of the Portuguese lectures on the *De caelo*, however, incorporate a brief exposition of the materials contained in the *Sphaera*, and do so immediately after their treatments of Aristotle's teachings. A representative text is that contained in the anonymous BN Lisbon FG 4066, dating from 1582, which reads as follows:

22 (Note 4) pp. 87-89.

BN Lisbob FG 4066.Anon. Annotationes in sphaeram Ioannis de Sacro Bosco sive Busto

[Fol. 115v] Mathematica scientia in quatuor partes a veteribus distributa [distributa a veteribus] est, in Arithmeticam [scilicet], Geometriam, Musicam, et Astrologiam. Astrologia[m] vero [generalis] vero ab astris nomen accepit; apud veteres comprehendebat Astronomiam et eam partem quam nunc speciali nomine vocamus astrologiam. Astronomia est doctrina quae per Geometriam et Arithmeticam inquirit atque demonstrat varios motus, magnitudines, numerum, atque distantias corporum caelestium, et quae omnes diversitates atque vicissitudines apparentiarum in planetis et reliquis astris observat [diservat]. Astrologia vero specialis est doctrina quae ex stellarum virtute natura [natura virtute] et situ diversas mutationes atque effectus in corporibus praedicit.

Hic igitur libellus spherae dictus [est] qui[a] continet tractationem de sphera, i.e., de corpore rotundo varios continente[s] circulos, editus quae est a Ioanne de Sacro Bosco, viro apud Parisienses in Astronomia plurimum versato ad astronomiam pertinenti [pertinet]. Continet enim prima elementa eius doctrinae quae dicitur astronomia. Continetur autem quatuor capitibus in quorum primo explicantur astronomiae principia sive fundamenta; in secundo declarantur [explicantur] circuli ex quibus sphaera materialis componitur, quam Archimedes Syracusanus ut nobis caelorum compositionem ob oculos proponeret dicitur invenisse: quoniam vero duplex est consideratio motus caelestis, altera qua intelligitur motus primi mobilis, altera quae de inferioribus motibus diseritur: agit autor in capito tertio de iis quae fiunt motu primi mobilis utpote de ortu et occasu signorum quae a primo mobili deferuntur, de [et] diversitate dierum et noctium quae ob diversum ortum et occasum signorum in diversis locis cernitur, de divisione climatum in quibus huiusmodi diversitas reperitur; in quarta denique de orbibus planetarum deque horum motibus et [idipsum ratione] eclipsium re pertractat.

De divisione sphaerae mundi
De partibus mundi
De numero caelorum
[De motu caelorum]
Caelum moveri circulariter et esse figurae sphericae
Terram esse centrum mundi
Ambitum terrae, et ex eo diametrum invenire
Caput secundum seu secundus liber:
De zodiaco
De coluris
De meridiano et orizonte
Quo artificio deprehendatur latitudo
De latitudine terrae a veteribus comperta
De longitudine
Quo artificio longitudo comperiatur
Quanta fuerit orbis longitudo a veteribus comperta
De circulo meridiano
De quatuor minoribus circulis
De quinque zonis

Pridie Kalendis Maii 1583 [Finis scripturae pridie callendas Maii feliciter impositus est 1583][23]

This same text is found in BU Coimbra Cod. Res. 2414 dating from 1582, with changes as noted above in square brackets. It is likewise found in BN Lisbon FG 2533, which contains the course on natural philosophy taught by Manuel a Lima in 1588-1589.

An interesting point in Galileo's notes on *De caelo* (MS Gal. 46) is the ambivalent attitude he manifests towards the existence of *influentiae* coming from the heavens and affecting the sublunary regions.[24] This same attitude is

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[23]*English translation:* Annotations on the *Sphaera* of John of Sacro Bosco or Busto.

Mathematical science has been divided into four parts by the ancients, namely, into arithmetic, geometry, music, and astrology. [General] astrology takes its names from the stars [*ab astris*]; among the ancients it included astronomy and the part we now give a special name, astrology. Astronomy is the teaching that uses geometry and arithmetic to inquire into and demonstrate various motions, magnitudes, numbers, and distances of the heavenly bodies, and that observes all the differences and changes in the appearances of the planets and other stars. Special astrology is the teaching that predicts various changes and effects in bodies from the nature, power, and arrangement of the stars.

This little book of the sphere is called such because it contains a treatise on the sphere, that is, a round body containing various circles, edited by John of Sacro Bosco, a man among the Parisians [working] in astronomy, who is very well versed in matters pertaining to astronomy. For it contains the first elements of the teaching that is called astronomy. It encompasses four chapters, the first of which explains the principles and fundamentals of astronomy; in the second are explained the circles of which the material sphere is composed, which Archimedes of Syracuse is said to have invented in order to set before our eyes the composition of the heavens. And because there is a twofold consideration of the celestial motion, one by which the motion of the first mobile is understood, the other, which is concerned with the inferior motions, the author treats in the third chapter matters that concern the motion of the first mobile, such as the rising and setting of the signs that are carried about by the first mobile, the difference of days and nights which is discerned from the various risings and settings of the signs in different places, and the division of the climes in which this diversity is found. In the fourth [chapter], finally, [he treats] of the orbs of the planets and their motions, and also the matter and explanation of eclipses.

[First Book:] On the division of the sphere of the world; On the parts of the world; On the number of the heavens; [On the motion of the heavens]; That the heavens move in a circle and are of spherical shape; That the earth is at the center of the world; The size of the earth, and from this to find its diameter.

[Second Book:] On the zodiac; On colures; On the meridian and the horizon; On the device used to determine latitude; On the latitude of the earth found by the ancients; On longitude; On the device used to determine longitude; The size of the longitude of the orb found by the ancients; On the meridian circle; On the four minor circles; On the five zones. The day before the Kalends of May 1583.

[24]*Galileo's Early Notebooks* (note 3), pp. 228, 297.

reflected in the writings of Vallius, who denied their existence outright, and of Vitelleschi and Rugerius, who held their existence as less probable.[25] By contrast, two sets of Portuguese lecture notes on Aristotle's *Meteorologica*, BU Coimbra Cod. Res. 2414 and the BN Lisbon FG 4921, hold for their existence. The first four questions of the Coimbra manuscript, which dates from 1582, read as follows:

> BN Coimbra Cod. Res. 2414. Commentarii in libros *Meteorologicorum* Aristotelis
> [Fol. 223v] Inscriptio
> [Fol. 223v] Utrum ad actionem horum inferiorum necessarium sit corpora caelestia in hunc mundum inferiorem agere
> [Fol. 225r]Sitne opus moveri caelum ut in haec inferiora agere valeat
> [Fol. 226r] Utrum caelum non solum motu sed lumine etiam ex influentiis in rebus inferioribus agat
> [Fol. 227r] Responsio affirmativa... Luna est causa fluxus et refluxus maris non solum cum movetur etiam quando latet apud Antipodas, sed tunc temporis lumen ipsius non potest ad nos pervenire; ergo alia quadam qualitate interveniente occulta nobis efficit fluxum et refluxum qui scilicet ad nostrum mare pervenerunt
> [Fol. 227v] Utrum motus sit causa caloris...[26]

The Lisbon manuscript, containing the lectures of Marco de Moura given in the academic year 1588-1589, has almost the same readings for the first four questions:

> BN Lisbon FG 4921 Annotationes in libros *Meteorologicorum*
> Utrum ad actionem horum inferiorum naturalium sit corpora caelestia in hunc mundum inferiorem agere
> Sitne opus caelum moveri ut in haec inferiora agat
> Utrum caelum non solum motu sed lumine et influentiis agat in haec inferiora
> ...Secunda conclusio. In caelo non modo ponendus est motus et lumen, sed peculiares aliae virtutes nostris sensibus occultae quae influentiae dicuntur quibus

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[25] Ibid., pp. 297, 313-314.

[26] *English translation:* Inscription; Whether for the action of inferior [i.e., sublunary] bodies it is necessary that celestial bodies act on the sublunary world; Is it necessary for the heavens to move so as to act on these sublunaries.

Whether the heavens act on sublunary things not only by motion and illumination but also by influences. Affirmative response... The moon is the cause of the ebb and flow of the sea not only when it moves but also when it lies at the Antipodes; but at that time the light of the moon cannot reach us; therefore some other occult quality must come between it and us to bring about the ebb and flow that reaches all the way to our sea [i.e., the Mediterraneum].

Whether motion is the cause of heat.

apud has res inferiores varios effectus pro ducit. Haec conclusio est D. Thomae secundo *De caelo*...

Utrum motus sit causa caloris

...Dicendum est igitur motum localem praecise sumptum non esse proprie per se causam caloris sed esse dispositionem quandam ad quam sequitur calor. Quod si quaeras quaenam igitur sit causa quae proprie et per se calorem producat, respondendum est unumquodque corpus habere calorem virtualem seu potentiam calefaciendi, eodem fere modo quo in pipere concedimus vim calefaciendi non quodvis subiectum sed quod certis dispositionibus affectum est, ut hominis linguam et stomachum.[27]

On the other hand, a third manuscript, the anonymous BU Coimbra Cod. Res. 2312 dating from 1580, omits these four questions and begins immediately with the proper subject matter of the *Meteorologia*, namely, the various dispositions of the earth's atmospheric region. Perhaps this is an indication that its author was not convinced of the existence of *influentiae* and so decided to omit them from his treatment.

These then are some excerpts from the materials I copied from manuscripts in Lisbon and Coimbra in 1977[28]. They are important for showing the presence of calculatory techniques in expositions of the *Physics* and for treating matters in the *De caelo* and *De generatione* that have counterparts in Galileo's physical questions now conserved in MS Gal. 46. In this sense they show that Jesuit teachings in Portugal in the 1580's were not very different from those at the Collegio Romano. Shortly after that time it seems that less emphasis was being put on the physical sciences, and particularly those parts

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27 *English translation:* Annotations to the books of the *Meteorologica*.

Whether it is [necessary] that the heavenly bodies act on our sublunary world so as to enable sublunary natures to act. Must the heavens move in order to act on these sublunaries.

Whether the heavens act on these sublunaries not only by motion but also by illumination and influences. ...Second conclusion. Not only must motion and illumination be posited in the heavens, but also those peculiar powers that are hidden from our senses and are called influences, whereby they produce various effects on sublunary things. This is the conclusion of St. Thomas in the second book of *De caelo*.

Whether motion is the cause of heat. ...It should therefore be said that local motion precisely taken is not the proper cause of heat, but that it is a certain disposition from which heat follows. And if you inquire what therefore is the cause that properly and directly produces heat, the response would be that each body has a virtual heat or power of heating, almost in the same way that we grant that there is a power in a pipe of heating not any subject whatever, but only [a subject] that is endowed with certain dispositions, such as those found in man's tongue and stomach.

28 For a fuller development of the themes discussed in this essay, see my "Domingo de* Soto and the Iberian Roots of Galileo's Science," forthcoming in *Hispanic Philosophy in the Age of Discovery*, The Catholic University of America Press, Washington, D.C. 20064.

that made use of mathematical reasoning. This newer direction is reflected in the *Cursus philosophicus* published at Coimbra between 1592 and 1605. Since this quickly became the standard philosophical text in Jesuit institutions of learning throughout the seventeenth century, it is not surprising that this aspect of Jesuit teaching – the one that had significant influence on the young Galileo – has consistently been overlooked by scholars. But the record still exists, conserved in the manuscript tradition and waiting to be uncovered by those who have the ability, and the patience, to search it out.

V

DUHEM AND KOYRÉ ON DOMINGO DE SOTO

ABSTRACT. Galileo's view of science is indebted to the teaching of the Jesuit professors at the Collegio Romano, but Galileo's concept of mathematical physics also corresponds to that of Giovan Battista Benedetti. Lacking documentary evidence that would connect Benedetti directly with the Jesuits, or the Jesuits with Benedetti, I infer a common source: the 'Spanish connection', that is, Domingo de Soto. I then give indications that the fourteenth-century work at Oxford and Paris on *calculationes* was transmitted via Spain and Portugal to Rome and other centers where Jesuits had colleges, and figured in the rise of mathematical physics at the beginning of the seventeenth century. A result of these researches is their vindication of Duhem, as contrasted with Koyré, on the origins of modern mechanics.

Pierre Duhem and Alexandre Koyré, both eminent French historians of science, held radically different views of the importance of Domingo de Soto for the evolution of modern science. For Duhem, Léonardo da Vinci was the linchpin in a development that stretched from the *Doctores Parisienses* to Soto, and Soto himself was the proximate source of Galileo's early writings and of the ideas contained in his later works (Duhem 1906–1913). Duhem based his analysis on two of Galileo's early manuscripts, now numbered 46 and 71 in the Galileo Collection in Florence, which had been transcribed and published by Antonio Favaro in the National Edition of 1890 with the titles *Juvenilia* and *De motu* respectively. For Koyré, on the other hand, Soto was merely an enigma, a Spanish scholastic isolated from the main flow of European thought (Koyré 1964). In his view neither Soto nor the Parisian doctors nor Léonardo figured importantly as sources of Galileo's science. Following Favaro's lead, Koyré preferred to see the whole of that tradition summarized in the writings of two of Galileo's Italian predecessors, Francesco Buonamici and Giovan Battista Benedetti (Koyré 1939, 1978). The first he discerned behind Galileo's MS 46 and the second behind his MS 71. The medieval and Renaissance development that had been traced in such detail by Duhem might be of antiquarian interest, but it was not at all necessary for Koyré's understanding of Galileo and the 'new science' he had brought into being.

Some years ago, at a conference in this Center, I focused on the*

debate between Duhem and Favaro as recorded in Favaro's 1916 review of Duhem's *Études sur Léonard de Vinci* and his 1918 resumption of that review in an essay entitled 'Galileo Galilei e i Doctores Parisienses' (Favaro 1916, 1918). My conclusion then was that, eminent though both were as scholars, neither had gone far enough in his researches; if they had, the dispute between them could have been dissolved in terms of what I was then developing as a 'qualified continuity thesis' (Wallace 1978, 1984b). In this essay I wish to enlarge on that theme by focusing not on Favaro but on Koyré, and by doing so in light of a third Galileo manuscript that was completely misjudged by Favaro, excluded by him from the National Edition, and as a consequence was unknown to both Duhem and Koyré. I refer to MS 27, the manuscript containing Galileo's treatises on Aristotle's *Posterior Analytics*, recently transcribed and edited by William F. Edwards and myself (Galilei 1988). This manuscript provides yet stronger support for Duhem's continuity thesis – but in a way that is somewhat surprising in that it allows one to integrate Koyré's findings into it and so include Benedetti as another possible link between Galileo and Soto. The intermediary that makes the linkage possible is the one that enabled me to dissolve the Duhem–Favaro controversy well over a decade ago, namely, the Jesuit tradition at the Collegio Romano. It is clear now that Galileo's MS 27 derives from lectures given at the Roman College, and, in light of that derivation, that MSS 46 and 71 derive similarly from the same source (Wallace 1986). What is more problematical is how to relate Benedetti to the Roman Jesuits. I shall therefore start with the Benedetti–Jesuit relationship and then work back from this to Domingo de Soto.[1]

BENEDETTI AND JESUIT SCIENCE

At first glance there would seem to be little that would connect the Collegio Romano, the Jesuit university founded by Ignatius Loyola in Rome in 1551, with Giovan Battista Benedetti, the patrician of Venice whose life spanned the years from 1530 to 1590. Benedetti's visits to Rome apparently were few. He is recorded as having lectured there on the science of Aristotle in the winter of 1559–1560, when the Collegio was but a fledgling institution, but to my knowledge had no contact with Jesuits at that time. From the Collegio side, in the years up to Benedetti's death there seems to have been little appreciation of his

scientific work on the part of its philosophy professors, although he was known to the eminent mathematician on the faculty, Christopher Clavius. Such tenuous connections offer little basis for a documentary analysis of possible ties between Benedetti and the Roman Jesuits (Wallace 1987b, nn. 1–5).

In the absence of such evidence, I shall turn to a conceptual study and focus instead on the role of mathematics in the study of nature as an apt basis for comparison. In it I aim to show that by the time of Benedetti's death in 1590, the faculty at the Collegio Romano had come to a view of mathematical physics very similar to his. This would seem to be an important consideration, for it was such a conception of mathematical physics that channeled into MSS 27, 46, and 71 of the young Galileo and thence exerted an influence on the development of his science. Thus the *terminus ad quem* of my investigation is Galileo's writings on motion and mechanics around the year 1590. The *terminus a quo* is somewhat more problematical, and I will come to that later. For the moment I shall identify it simply as 'the Spanish connection', based on the facts that, on the one hand, Benedetti's father was a Spanish philosopher and physicist (or physician) and that many of his own professional contacts were with Spaniards, and, on the other, that the early Jesuit professors at the Collegio Romano were also Spanish and imported from the Iberian peninsula several ideas that proved seminal in the new mathematical physics (Wallace 1987b, nn. 6–9).

Starting, then, in somewhat ahistorical fashion with the *terminus ad quem*, let me characterize briefly the concept of science that emerges clearly in Galileo's early treatises on motion in MS 71 and that continued to dominate his later writings down to the *Two New Sciences* of 1638. This was very much a mathematical physics that proposed itself as a *scientia* and presented its reasonings in the form of *demonstrationes*; its model was ostensibly that of Archimedes, but the ideal was already Aristotle's as formulated in his *Posterior Analytics*. Working out the implications of his new *scientia* (in effect a *scientia mixta* or *scientia media*, subalternating physics to mathematics), Galileo was sharply critical of the causal analyses found in Aristotle's *Physics* and *De caelo*, while at the same time he was intent on searching out, in an Aristotelian mode, the *verae causae* of natural phenomena. Local motion (*motus localis*) was his major concern; to explain this he invoked the principal concepts used by Aristotle – nature and violence, time, place and space, force and resistance, causality – although he rejected others associated

with the medium through which the moving object passed, e.g., Aristotle's teaching on the void and his solution to the projectile problem. In their place Galileo substituted the scholastic concept of *impetus*, which he used to explain both violent and natural motion. His most important methodological innovation was his clever use of *suppositiones* when framing his demonstrations, making them amenable to the use of limit concepts and to applications in experimental situations where a mathematical ideal could be closely approximated in the physical world (Wallace 1987b, nn. 10–14).

Much of my recent research has been directed at showing how this view of science is indebted to the teaching of Jesuit professors at the Collegio Romano, whose lecture notes on logic and natural philosophy were the proximate source of Galileo's MSS 27 and 46 and prepared for the *De motu antiquiora* of MS 71. But those who are acquainted with the works of Benedetti will surely have noticed how closely Galileo's concept of mathematical physics just sketched by me corresponds to that of Benedetti. Such correspondence suggests points of comparison between Benedetti and the professors of the Collegio. To develop it, we must look in detail at Benedetti's main theses and then see how these compare with related teachings among the Jesuits whose lecture notes I have studied (Wallace 1987b, nn. 15–16).

BENEDETTI'S MATHEMATICAL PHYSICS

For convenience let me divide my consideration of Benedetti's physics into two parts, the first concentrating on its logical and methodological foundations, the second on its treatment of problems relating to local motion. With regard to the first, there can be no doubt that, from the outset of his career, Benedetti wished to reinforce his arguments as much as possible with 'mathematical demonstrations' (Benedetti 1553); in his last and most important work he identifies his basic disagreement with Aristotle as based "on the unshakable foundation of mathematical philosophy, on which I always take my stand" (Benedetti 1585, p. 196). This presupposes, of course, a difference between physics and mathematics, of which Benedetti was well aware: "balances or levers are not pure mathematical lines", he writes, "but are physical, and as such exist in material bodies" (144).[2] Again, "since balances are material and are sustained . . . not by a mathematical point but by a line or a physical surface having a material existence, some resistance arises to the motion of the arms" (153). Yet he wished to use mathematical

principles, such as that "a sphere touches a plane at only one point" (155), to establish physical conclusions. The only way he could do this, he recognized, was through the use of appropriate suppositions and thought experiments. It is thus important to recognize how frequently the terms *suppositio* and *imaginemur* (and their variants) recur in Benedetti's writings. Well known are his disagreements with Tartaglia and Jordanus Nemorarius in his solution of mechanical problems; invariably these are occasioned by the divergent *suppositiones* on which the respective solutions are based. For example, Benedetti frequently reproves Tartaglia for supposing that the "lines of inclination" going from the ends of a balance to a distant center of gravity are parallel (150). Yet on some occasions he makes the same supposition himself, noting that the line of inclination is *fere perpendicularis* to the beam of the balance or that, if the angle it makes is not a right angle, the deviation is negligible. But, when discussing the imagined case of a balance situated close to the earth's center, he rightly insists that the approximation cannot be made and that the simplifying supposition cannot be employed in a rigorous proof (143).

Such suppositions are important in the treatment of problems in statics, but they are crucial for the development of a science of dynamics. Benedetti was intent on discovering the *verae causae* – an expression that occurs repeatedly in his writings – of various kinds of motion in the universe, both natural and forced. An important contribution was his study of horizontal motions on the earth's surface; here he was convinced that the only truly natural motion is circular, for this alone can be perpetual (184). But, he reasons, there is "no noteworthy difference" between a perfect sphere and a plane surface of small extent. For this reason one will encounter no difficulty in moving a sphere along a horizontal surface; indeed, it can be moved by "a force no matter how small" (156). In another context he qualified an argument to specify that it holds only "when all impediment is removed" (154). Such insights, plus Benedetti's frequent allusion to the natural tendency of a body when released from a sling to move in a straight line, shows how close he came to the principle of inertia later formulated by Sir Isaac Newton.

Moving on to his study of problems relating to local motion, we can treat these under three headings, namely, those relating to motion in general, those relating to falling motion, and those relating to the movement of projectiles. With regard to the first, Benedetti was Aristotelian in his conviction that nature is an inner source of motion in a

body; even forced or violent motion he saw as caused by an *impetus* or *virtus movens* impressed within a body. But unlike Aristotle he seems not to have invoked a sharp dichotomy between natural and violent motion, or between curvilinear and rectilinear motion, regarding the latter two as mathematically comparable (194). He does not discuss explicitly the possibility of a *motus medius*, i.e., one intermediate between the natural and the violent, but for him horizontal motion for limited distances would answer to that description. And in the case of reflex motion, he invokes the principle that a circle touches a line at only one point to argue that no intermediate rest (*quies medius*) interrupts the upward and downward motion of a body, making its motion truly continuous (184).

Benedetti is most known, of course, for his study of falling motion, especially for his argument *contra Aristotelem et omnes philosophos* that velocity of fall is dependent not on weight but on specific gravity, and therefore is conditioned by the medium in which the body falls and the resistance it encounters (Maccagni 1983). He proposed that velocity of fall increases with distance of travel because impetus builds up naturally in the falling body, and that all bodies would fall with the same speed *in vacuo*, where buoyancy and resistive effects can be neglected. Gravity and levity became for him relative concepts, so that air has no weight in air, nor water in water. And he analyzes the case of a body falling through the center of the earth to argue that it would oscillate about the center, on the analogy of the motion of the bob of a pendulum of exceedingly long length (Benedetti 1985, pp. 174–85, 368–69).

Equally ingenious is Benedetti's study of projectile motion, which is dominated by his skillful use of the concept of impetus, already referred to. This he regarded as a force impressed on a body from without but that moves it from within, decreasing gradually and continually with the body's motion (160). Most motions involving trajectories of bodies he saw resulting from a composition of motions, partly natural and partly forced (161), and in this is seen as having noticeably advanced beyond the teaching of Tartaglia.

COUNTERPARTS IN JESUIT TEACHINGS

Such was the contribution of Benedetti to the foundations of mathematical physics by the time of his death in 1590. The question I now would

raise is this: how similar were the teachings of Benedetti as I have just outlined them to those at the Collegio Romano during the years, say, from 1560 to 1590? An answer is difficult because of the paucity of records that have survived from this period. At the beginning, Francisco Toletus taught the physics course in Rome during the academic year 1560–61, and his printed text gives indication that his ideas were fairly similar to Benedetti's. But only a year or two later, Benedictus Pererius took over the course in 1562–63, and, as his textbook indicates, set it on a path almost diametrically opposite to his predecessor's. Decidedly antimathematical and Averroist in his approach, Pererius combatted most of the Benedetti's theses concerning motion, not naming him explicitly or even being aware of his teachings, but simply rejecting out of hand the principles on which they were based (Giacobbi 1977).

This mentality apparently persisted at the Collegio for some fifteen years, and then gradually changed owing to two factors: the influence of Clavius, who fought strenuously to give mathematics a respectable place in the curriculum, not merely in its own right but also as an adjunct to natural philosophy; and the advent of a new physics professor, Antonius Menu, who was much interested in 'calculatory' techniques and imported them where possible into his lectures. A series of professors who followed Menu – Paulus Vallius, Muzius Vitelleschus, and Ludovicus Rugerius – developed their teachings on motion and the heavens along lines more acceptable to Clavius, and thus closer to Benedetti. Finally one of Clavius's special students, Giuseppe Biancani, synthesized all this work by systematically elaborating a mathematical physics capable of dealing with the problems of natural philosophy (Giacobbi 1976).

I shall elaborate more fully on this development later in the essay. Suffice it here to call attention to Vallius's commentary on the *Posterior Analytics*, particularly to his treatise *De praecognitionibus*, which was appropriated by Galileo in his MS 27 (Galilei 1988); this, taken in conjunction with Clavius's preface to his *Elements* and Biancani's later emendations, shows how *suppositiones* can be employed to uncover causes and supply *demonstrationes* in these difficult subject matters. Menu and Vallius recovered the concept of *impetus* and showed how it, and other notions in the scholastic tradition, could improve Aristotelian teachings as these were being advanced by the peripatetics of their day (Wallace 1981c). Vitelleschus and Rugerius built on these foundations. Vitelleschus is particularly important for his awareness of Benedetti's analysis of falling motion, though he knew it only through a work

by Jean Taisnier that plagiarized Benedetti's *Demonstratio* of 1554 (Maccagni 1967). The reference occurs in Vitelleschus's lectures on the *De caelo* of Aristotle (given in 1590), where he questions Aristotle's laws of motion as stated in Books 4 and 7 of the *Physics*, and directs his students to the treatises of Bradwardine and Taisnier on the ratios of motions. In the same manuscript Vitelleschus echoes Benedetti's sentiment against the authority of Aristotle, stating that it is safer to abandon some of his teachings than it is to interpret them, for the authority of a philosopher should be used to confirm the truth, not abandon it, seeing that truth is the philosopher's friend. Rugerius then took up Vitelleschus's teachings on the ratios of motion, noting that Aristotle's rules for comparing motions labor under severe difficulties. For a fuller discussion of how they might be revised he then refers his students to the commentaries of Toletus and Soto, among others, in their commentaries on the *Physics* (Wallace 1987b, nn. 53–57).

In the writings of Jesuit professors from Menu to Rugerius, therefore, one can find illuminating discussions of the internal causes of motion, of the possibility of motion in a void, and of an intermediate or neutral motion (neither natural nor violent) that can endure perpetually. One can find too a rejection of the *quies medius* in reflex motion; a rejection of Aristotle's dynamical laws of motion; a sophisticated discussion of gravity, including the distinction between extensive and intensive gravity, similar to Benedetti's notion of specific gravity; a rejection of the notion that air has weight in air based on Archimedian principles; and detailed analyses of the factors that cause bodies to accelerate as they fall. Not all these teachings are the same as Benedetti's, but one gains the impression that, had the Venetian mathematician visited the Collegio in the years following the publication of his last work, he would have found a compatible atmosphere in which to advance his researches (Wallace 1987b, nn. 58–59).

THE SPANISH CONNECTION

This, then, brings me back to *terminus ad quem* with which I began this discussion. Most of the ideas I have just sketched are to be found, in various ways, in the lectures of Jesuits in Rome around 1590, in Galileo's MSS 27, 46, and 71, likewise composed around 1590, and in Benedetti's publications, probably known to Galileo through Jacopo Mazzoni, with whom he studied in 1590. I suspect that it was a fusion

of ideas gleaned from Benedetti and the Jesuits that lay behind the various drafts on motion in Galileo's MS 71. Yet I have found no documentary evidence that would connect Benedetti directly with the Jesuits, or the Jesuits with Benedetti, in the development of these concepts. Was there a common source from which they could have derived? I suspect that there might have been, and I would like to speculate about this as the *terminus a quo* of my investigation – the 'Spanish connection' to which I have alluded above.

A plausible candidate for the origin of a mode of thought that would allow mathematics to enter into an experimental study of motion is none other than the Spanish Dominican who first proposed that the motion of falling bodies is uniformly accelerated with respect to time – *uniformiter difformis* is the expression he used – and who was seen by Duhem on this account to be a scholastic precursor of Galileo (Duhem 1906–1913). I refer, obviously, to Domingo de Soto. Soto was known to the Jesuits; indeed, Toletus had studied under him at Salamanca before joining the faculty of the Collegio, and Rugerius, as we have seen, called attention to his superior treatment (along with Toletus's) of Aristotle's dynamic laws of motion. Now, in his commentary and his questions on the *Physics*, Soto assimilated his doctrine on impetus to his teaching on *gravitas* and taught that a falling body accelerates continuously because of the impetus being built up in it during its travel – ideas very similar to those found in Benedetti. These notions are not fully developed in an incomplete edition of Soto's *Physics*, published at Salamanca around 1545, but they are present in the edition of 1551 as well as in the more widely diffused second edition of 1555, both also printed in Salamanca. Between 1545 and 1550, moreover, Soto was present at the Council of Trent, which took place just north of Venice. As the most illustrious theologian in the Dominican Order, he was surely known to Abbot Gabriel de Guzman and the two Spanish Dominicans Benedetti praises so lavishly in his *Resolutio* of 1553 and his two versions of the *Demonstratio* of 1554 and 1555, directed, as we saw, "against Aristotle and all philosophers". While in northern Italy, it is also possible that Soto became acquainted with experimental work being done there on laws of fall, which would have buttressed his own rejection of Aristotle's teaching on this subject. And finally, though I have found no mention of Soto in Benedetti's *Speculationes* of 1585, it may be no mere coincidence that Soto's *Physics*, both commentary and *quaestiones*, was reprinted in Venice in 1582 with an introduction that

gives fulsome praise to his ability as a natural philosopher (Wallace 1987b, nn. 63–68). All bits of coincidental evidence, but all pointing to Soto as a link that could ultimately tie together Benedetti, Galileo, and the Jesuits of the Collegio Romano.

SOTO'S SECOND ENIGMA

Earlier I remarked that Soto was an enigma for Koyré, but I did not elaborate on Soto's enigmatic status. Actually two enigmas can be associated with Domingo de Soto. The first is how he came to know that the motion of heavy bodies in free fall is *uniformiter difformis* with respect to time, and the second is how this knowledge might have been transmitted to Galileo. The first enigma was what puzzled Koyré and served as the subject of an essay I published years ago with the title * 'The Enigma of Domingo de Soto: *Uniformiter Difformis* and Falling Bodies in Late Medieval Physics' (Wallace 1968). The second enigma, to my knowledge, was not explicitly addressed by Koyré, although it posed the problematic on which much of his *Études galiléennes* was based. Let us address this second enigma now, for, if we can cast light on that, we may additionally be able to fill a lacuna in Duhem's thesis about Soto and his importance for Galileo's science. We can do so through a study of books and manuscripts written by Jesuits in Italy and Portugal in the century following Soto's publication of his *uniformiter difformis* doctrine.

Galileo mentions Soto twice in MS 46, in a *Tractatus de elementis* that occupies the last part of the manuscript. We now know that this *Tractatus*, as well as other treatises written by Galileo at Pisa around 1589–1591, were based on lectures given by the Jesuits mentioned above (Wallace 1977). Some of these lectures were published, but the majority survive only in manuscript. They were based on scholastic and Renaissance authors, whom they cite extensively, and are otherwise prosaic teaching notes. What makes them somewhat distinctive is the attention they pay to nominalist teachings deriving from the *Calculatores* of Oxford University and the *Doctores Parisienses*.

The development of these lecture notes took place in Rome at the Collegio Romano over a period of some thirty years. There the introduction of calculatory thought is traceable to Toletus, himself a Span-

iard, who taught the course in natural philosophy in 1560 and imported ideas he had learned from Soto at Salamanca. Some of this material was taken up by two other Spanish Jesuits, Pererius, mentioned above, who taught natural philosophy at the Collegio between 1558 and 1566, and Francisco Suarez, who taught theology there between 1580 and 1585. Fortunately these authors published their materials, which have been analyzed in some detail by Christopher Lewis (Lewis 1980). Lewis picked out for examination the use by all three of calculatory language in the following areas of natural philosophy: (1) when discussing problems of action and reaction; (2) when treating the intension of forms in alteration and identifying distributions of qualities as uniform, uniformly difform, etc.; and (3) when analyzing the ratios of motions following the tradition of Thomas Bradwardine.

Of the three Jesuits, Toletus undoubtedly made fullest use of the *Calculatores* in these areas, even referring to "the calculator Suisset" (i.e., Swineshead) by name in his treatments of reaction and alteration. He also had the clearest understanding of calculatory terminology, although he frequently departed from positions held at Oxford and favored instead those developed at Paris. In treating expressions such as *uniformiter difformis*, moreover, Toletus made the interesting comment that "these [terms] should be very carefully considered in order to understand many matters that are met with in physics". Suarez likewise took up uniform difformity in some detail when analyzing the action of natural agents in his *Disputationes metaphysicae* of 1597, although he rejected the view (apparently subscribed to by Toletus) that velocity could be viewed as an intensity of motion, which would be expected of one subscribing to Mertonian developments in kinematics. Pererius, predictably, showed the least acquaintance with, or interest in, the calculatory tradition, although he was acquainted with some of its terminology. In discussing the dynamical laws given by Aristotle in the seventh book of the *Physics*, for example, Pererius accepted and defended them without even a nod in the direction of Bradwardine, thus showing little sympathy for the mathematical physics developed two centuries earlier at Merton College, Oxford (Lewis 1980).

As already noted, partially because of his antimathematical bias Pererius was replaced after 1566 and succeeded by a series of other professors. Lecture notes survive from only two who taught between then and 1585, viz., Hieronymus de Gregorio and Antonius Menu, but the second of these, Menu, enjoyed the longer tenure and seems to have

had the greater influence. Menu revived the approach of Toletus and had a notable effect on the Jesuits mentioned above who taught natural philosophy at the Collegio between 1585 and 1592, namely, Vallius, Vitelleschus, and Rugerius, all of whose lecture notes survive in whole or in part. Although some details are lacking, these four professors supplied the materials on which Galileo's early notebooks on the *De caelo* and *De generatione* and the early versions of his *De motu* were based, and so serve to explain Galileo's knowledge of the calculatory tradition (Wallace 1987a, n. 14).

Menu is of particular importance for standing at the head of this fifteen-year tradition, which used Mertonian terminology but usually applied it in ways more consistent with teachings in vogue at Paris in the fourteenth century and so associated with the *Doctores Parisienses*. Indeed, Menu cites these doctors when treating the question whether the world could have existed from eternity and when discussing the ratios of the elements. He was also favorable to their adoption of impetus, or *virtus impressa*, as necessary to explain the motion of projectiles. Particularly striking are his arguments in favor of the proposition that "the motion of a simple or compound body through a void will be successive, for granted that it would encounter no extrinsic resistance, there would still be intrinsic resistance" to overcome. These are clearly those of the *Parisienses*, adopting the calculatory stance of the Mertonians but applying it to physical problems in the tradition of Jean Buridan, Albert of Saxony, and others who worked in fourteenth-century Paris (Wallace 1987a, nn. 15–16).

The lecture notes of Vallius, Vitelleschus, and Rugerius do not employ these particular arguments, but they nonetheless touch on all the matters pertaining to the mathematical or calculatory tradition that are to be found in Galileo's early writings. The latter's notes in MS 46 are written in the form of a questionary based on Aristotle's *De caelo* and *De generatione*. The questions wherein analytical languages in the Mertonian and Parisian traditions occur most frequently are in the treatises *De alteratione* and *De elementis*, where inquiries are made into the intension and remission of forms, the parts and degrees of qualities, and the number and quantity of the elements. There seems little doubt that all of these materials are derived from lectures given at the Collegio some time prior to 1591. The precise author is difficult to identify, however, since correspondences can be found between Galileo's notes and passages in Rugerius, Vitelleschus, Vallius, and Menu, and indeed

all the way back to Pererius and Toletus. At the present stage of research Vallius seems to be the most likely candidate, for, although his surviving lecture notes are incomplete, the portions that survive show closest agreement with Galileo's text. There is excellent evidence, moreover, to connect Galileo's MS 27, the one containing questions on Aristotle's *Posterior Analytics*, with Vallius's lectures on logic, which were completed in the summer of 1588 and manifest a good knowledge of nominalist positions on science and demonstration (Wallace 1987a, nn. 19–21).

My study of all these materials thus encourages me to go considerably beyond Christopher Lewis in identifying likely sources of Galileo's knowledge of the calculatory tradition. Suffice it to mention that citations from Walter Burley and William Heytesbury, as well as Bradwardine and Swineshead, are to be found in these Jesuit lectures. And not only do such citations occur in discussions involving intension and remission, latitudes of qualities, and maxima and minima, but they also occur in discussions of local motion and of Aristotle's dynamical laws involving ratios between forces, resistances, and velocities of motion. Vitelleschus, for example, cites experimental evidence against the Aristotelian formulations in Book 7 of the *Physics* and refers his students to Bradwardine's *De proportione motuum* for an alternative view. Rugerius likewise discerns difficulties with Aristotle's rules and, as already noted, sends his students to Toletus and Soto for more satisfactory treatments of the ways in which velocity varies at the beginning, middle, and end of motion (Wallace 1987a, nn. 22–24).

Of the natural philosophers who taught physics at the Collegio Romano after Rugerius down to 1626, I have thus far located *reportationes* of lectures by four other Jesuit professors: Robert Jones, an Englishman, who taught in 1592–93, Stefano Del Bufalo, who taught in 1596–97 and again in 1598–99; Andreas Eudaemon-Ioannis, who taught in the intervening year 1597–98, while Del Bufalo had the course in metaphysics; and Fabiano Ambrosio Spinola, who taught in 1625–26. Of these, the treatments of the first and the last, Jones and Spinola, show less concern with the calculatory tradition. Del Bufalo, on the other hand, has a rather full discussion of alteration, degrees of qualities, intension and remission of forms, and action and reaction – in the last of which he mentions the teaching of the *Calculator* and contrasts it with those of Pomponazzi, Buccaferreus, Flaminio Nobili, Franciscus Neritonensis, and Zabarella. In his discussions of *gravitas* and *levitas*,

moreover, he mentions the *Parisienses* and compares their teachings with those of Geronimo Borro and Buonamici – and this in 1597, the year in which Buonamici's *De motu* had just appeared. It is noteworthy that all of Del Bufalo's notes located thus far are in the National Library at Lisbon, where they had been taken from the Jesuit college at Evora, having been sent there from Rome by October of 1603, as recorded in one of the codices containing them (Wallace 1987a, nn. 25–28).

The other professor who deserves mention for his calculatory interests is Eudaemon, who, as already mentioned, had the course in natural philosophy in 1597–98. In addition to his lectures on the *Physics*, *De caelo*, and *De generatione*, he left a *tractatus* in two books on action and passion and a *quaestio* on the motion of projectiles, both of which are written in the calculatory manner. As I have pointed out in my *Galileo and His Sources*, Eudaemon is of some importance for the fact that he discussed "the ship's mast" experiment with Galileo at Padua, and, along with Biancani, also teaching there, could have influenced Galileo's use of calculatory terms in his *De motu accelerato* fragment and later writings (Wallace 1984a; 1987a, nn. 29–30).

The first book of Eudaemon's work on natural agency, entitled simply *Tractatus primus*, is prefaced by five definitions and nine suppositions; it then develops twenty-one propositions, with proofs and corollaries, all relating to the ways in which qualities come to be mathematically distributed as a result of such agency, with occasional geometrical diagrams in the margins illustrating the text. Noteworthy among the definitions are the third and the fifth, the third stating that "something is said to be distributed uniformly difformly when it diminishes in the same ratio as the distance increases," and the fifth explaining how quantitative attributes can be ascribed to a quality that is uniformly difformly distributed. Following the definitions Eudaemon begins his suppositions, which he prefaces with the note:

> Because the matters with which we shall be concerned are physical, it is necessary to take some propositions from our physical disputations that can be presupposed as principles in this treatise. Things that are commonly conceded in physical science or are sufficiently proved and explained may be seen in our disputations on *De generatione* and on the *Physics*. And since this treatise is principally mathematical, it will not be necessary to note and prove propositions that come from mathematics.

This notation, and the nine suppositions that follow it, are important for the fact that they show Eudaemon adopting the stance of a mathe-

matician and attempting to develop a mathematical physics of natural agency, even though he was a philosopher entrusted with the main sequence of courses at the Collegio. Also noteworthy is Eudaemon's first supposition, which reads as follows:

> We presuppose that every natural agent acts *uniformiter difformiter* on a quantified subject when applied to it. Physicists commonly concede this, at least with respect to some parts of the sphere of activity, because we see that when close the agent acts more vehemently and when farther way less so; therefore, the closer the greater, the farther the lesser; therefore, as the distance increases the action decreases; therefore the action is uniformly difform.

Noteworthy here and throughout the treatise is the preoccupation with the expression *uniformiter difformis* as applicable to natural agency (Wallace 1987a, nn. 31–33).

The second book of this same treatise is titled *De iis quae in actione et passione physica contingunt* and it begins, like the first, with definitions, ten in number, then notes a single supposition, and concludes with proofs of thirty-one propositions, some of which contain substantial numbers of corollaries. The reason for this development is not transparently clear at first reading, but it all becomes intelligible when we get to the *Quaestio de motu proiectorum* that follows immediately after the second book. The entire treatise on natural agency had occupied fifty-one closely written folios; that on the motion of projectiles coming after it takes up seventy-two more. Divided into three articles, it inquires first whether the projector moves the projectile immediately at a distance, then whether the *vis movens* is within the projectile itself, and finally whether the *virtus movens* is located in the medium, and if so, how (Wallace 1987a, n. 34). Somewhat surprisingly, considering the fact that his predecessors at the Collegio had all adopted the impetus explanation of projectile motion, Eudaemon ends up by rejecting an impetus in the projectile and by putting the *virtus movens* in the air. I have not yet analyzed his arguments in detail, but I suspect that his reason for doing so is to subsume projectile motion under natural agency so as to show that it slows down uniformly difformly. This, we may recall, was Soto's position, for he held that falling motion is accelerated and projectile motion decelerated in the same quantitative way, namely, *uniformiter difformiter*.

If such was Eudaemon's thesis in this manuscript, undated but proba-

bly written in 1599, his discussions with Galileo at Padua around 1604 take on special significance. At that time Galileo was looking for a principle on which he could construct his new science of motion, as we know from his letter to Paolo Sarpi. His telling Eudaemon that he had experimented with a stone dropped from the mast of a ship first at rest and then in motion shows that both were still interested in the problem of impetus. Eudaemon could therefore have been a source that directed Galileo's attention around 1604 to calculatory treatments of uniform acceleration and deceleration, later to be reflected in the *De motu accelerato* fragment on which the *Two New Sciences* would be based (Wallace 1987a, nn. 35–37).

Let us look back, then, at the situation at the Collegio Romano from the time of Pererius, say 1566, when he taught, or 1576, when his textbook was published, to Eudaemon in 1599. In all of that time there were many references to the *Calculatores* and *Parisienses* and how they impacted on theses in natural philosophy. Not one, however, is to be found in a printed text – all occur only in manuscript sources. It is not surprising, therefore, that influences deriving from this tradition have thus far been overlooked by scholars and so have not been seen as a significant factor in the growth of mathematical physics among the Roman Jesuits toward the end of the sixteenth century.

THE JESUIT TRADITION IN PORTUGAL

To move now to the Iberian peninsula, a situation similar to that at the Roman College existed in the Jesuit colleges there, particularly in those at Evora and Coimbra. The Coimbran *Cursus philosophicus* was a five-volume course, first published at Coimbra between 1592 and 1605 and reprinted often thereafter. My researches have shown that natural philosophy in Portugal became less technical and mathematical from the end of the sixteenth century onward, and this possibly explains why there is no conspicuous use of calculatory terminology in the famous *Cursus*. A goodly number of manuscripts from Evora and Coimbra dating from the 1570s and 1580s are still extant, however, and these show the same patterns deriving from the *Calculatores* and the *Parisienses* as do the lecture notes from the Collegio Romano.

Lectures on the *Physics* and *De caelo* for the years 1570, 1582, 1587, and 1588 by professors named Juan Gomez de Braga, Luis de Cerqueira, Antonio del Castelbranco, and Manuel a Lima respectively

are all extant. Some of these Jesuits taught at Evora, others at Coimbra, but the substance of their notes is all the same; in some cases the wording is repeated almost exactly, suggesting a transmission of notes from one professor to another. In addition, notes from a Trinitarian, Marcus de Moura, who taught at Lisbon in 1588 are available, and his lectures are substantially the same as those given by Manuel a Lima at Evora in the same year. The same could be said of an anonymous set of lectures on the *Physics*, *De caelo*, and *Meteorology* given at Coimbra in 1580 (Wallace 1987a, nn. 40–42).

The anonymous lectures of 1580 are a good place to start, for their author gives a key to the source of most of the materials the others contain. The fifth chapter of his commentary on the seventh book of the *Physics* begins with two questions: (1) whether the velocity of local motion is to be ascertained from the quantity of space it traverses as from an effect, and (2) whence the velocity of motion is to be judged as from a cause. Following his replies to these queries the author writes: "These last two questions are treated more fully by Domingo de Soto and can be studied there. For this reason, and especially because of limitations of time, we will pass over them quickly." And his reply to the first question indicates the extent of his dependence on Soto, which I give in the slightly clearer formulation of Cerqueira, who repeated this material at Coimbra two years later, in 1582:

> Sometimes the mobile is moved so difformly with respect to time that, taken [any] part of time in which it moves, the velocity it has at the middle instant will exceed the velocity it had or will have at one terminal instant of that time by the same amount as it is exceeded by the velocity it had or will have at another [terminal instant]. Such a motion is said to be *uniformiter difformis* with respect to time, and it is found in heavy and light bodies when they move naturally, since the more they depart from their starting point the greater is the velocity with which they move.

This, of course, is the teaching developed by Soto around 1550, which is reiterated in most of these lecture notes preserved in Portugal throughout the 1570s and 1580s. It is further explained and extended to projectile motion by Cerqueira, and by Manuel a Lima again in 1588, in the following terms:

> It is customary to ask at this place why it is that things that are moved naturally in rectilinear motion are moved more swiftly at the end than at the beginning of their motion, whereas those that are moved violently are moved more swiftly at the beginning . . . . The reason for this is that, just as the force that exists in the hand of the thrower when

conjoined with the stone . . . impresses on the stone a certain impulse that moves it when separated from the hand of the thrower, so also gravity and levity, impelling the heavy and light thing to its natural place, impresses by such motion a certain impulse through whose agency the motion of the heavy and light thing is made swifter. And this impetus gets more intense as the heavy and light objects come closer to their natural places, which is to be understood in terms of the relation of each to the *terminus a quo*. For if one and the same stone were now to descend from the middle of a tower and later from its top, it would descend much more swiftly at the end of the later motion than at the end of the earlier. For the longer the space that is traversed the greater is the impetus impressed by levity and gravity throughout the motion, since it is continually intensified until the thing arrives at its natural place. And since this impetus effects in the heavy or light thing a motion similar to that which arises in it from gravity or levity, Aristotle referred to it as an increase of gravity and levity; others, however, speak of it as accidental gravity and levity, since it is lost as soon as the motion stops.

I give this as one example relating to the ratios of motions; similar materials relating to action and reaction, wherein the opinions of Heytesbury and the *Calculator* are discussed, could also be mentioned. But perhaps this is sufficient for present purposes to show evidences of a Jesuit mathematical tradition on the Iberian peninsula in the latter part of the sixteenth century (Wallace 1987a, nn. 43–46).

To return briefly to Italy, I would add that Biancani, who had studied under Clavius at Rome in the 1590s, wrote detailed defenses and justifications of mathematics and mathematical physics as sciences in the Aristotelian sense, wherein he shows considerable competence as both a philosopher and a mathematician. These he explicitly directed against Pererius and the authors of the Coimbran *Cursus philosophicus*. Biancani taught principally at Parma, where Giambattista Riccioli was in turn his student. And Riccioli is of some importance for his verification of Galileo's experiments on falling bodies. In his *Almagestum novum* of 1651 Riccioli recounts that he had first started experimental work in this field with two other Jesuits in 1629, and then with yet another in 1634. At that time he obtained permission, he says, to read Galileo, whom he first thought to be in error but later found to be correct. Of his early work Riccioli writes that

at that time I had not yet come to the better and more evident experiments manifesting not only an inequality in the motion of heavy bodies but the true increment of their velocity, increasing *uniformiter difformiter* toward the end of the motion.

What is interesting here is Riccioli's use of Parisian terminology deriving from Soto when describing the results to which he had finally come. This seems to me a fairly good indication that such terminology had

been part of his training too, and persisted in his mind to the middle of the seventeenth century, i.e., to 1651 (Wallace 1987a, pp. 58–62).

CONCLUDING REMARKS

From all these indications it would seem that the fourteenth-century work at Oxford and Paris on *calculationes*, transmitted via Spain and Portugal to Rome and other centers where Jesuits had colleges, figured in the rise of mathematical physics at the beginning of the seventeenth century. The circumstances of this transmission may help to clear up two problems that have bothered historians of science. The first of these is the disparity between Galileo's use of calculatory language and that of the Mertonians, which has recently been analyzed by Edith Sylla (Sylla 1986). Such disparity is readily understandable when one considers that Galileo acquired that language at several removes from its initial formulators. The second is the lack of a consistent attitude on the part of the Jesuits toward the use of mathematics in the study of nature. This becomes intelligible in terms of the tensions that developed within the Order between the mathematicians and the philosophers, and the censorship that was invoked to present a 'united front' to the outside world. Vallius had difficulty with censors within his own Order when he attempted to have his Collegio Romano lectures on logic and natural philosophy published in the early 1600s, and we know that Biancani ran into the same difficulty with censorship when he wrote in 1615 and 1620 in support of Galileo (Wallace 1984a, pp. 141–48). Invariably the theologians and the leadership within the Order sided with conservative confreres among their philosophers rather than with progressive confreres among their mathematicians whenever Church teaching was involved. As a result, the period between about 1560 and 1650 presents a somewhat ambivalent picture of the Jesuits' role in the development of mathematical physics. But the manuscript record, official positions aside, shows that solid progress was being made during those decades, wherein foundations were laid for later important contributions to the sciences from within the Society of Jesus.

A yet more important result of these researches for this conference is their vindication of Duhem, as contrasted with Koyré, in the work of these French historians on the origins of modern mechanics. Koyré's fortunes have declined in recent years with the discoveries by Stillman Drake and others of the extensive experimental program on which

Galileo had embarked between 1604 and 1610. This has sounded the deathknell for Koyré's appraisal of Galileo as a Platonist or rationalist who had no need of experiment to found his 'new science'. My own work tarnishes Koyré's image a bit more, for it shows that his neglect of medieval and scholastic sources vitiated much of the reasoning behind his *Études galiléennes*, the part relating to Benedetti alone excepted. But if Koyré has been devalued, as it were, the same cannot be said of Duhem. Duhem's emphasis on Soto, it turns out, was well founded. One would no longer wish to maintain that Soto was the proximate source of Galileo's science. But whether one traces Soto's influence through the Jesuits in Italy or in Spain and Portugal, or by a parallel route through Benedetti, there seems little doubt that Soto played a pivotal role in promoting a mathematical analysis of local motion.[3]

NOTES

[1] The further development of this essay is a conflation of two previously published studies, the first focusing on the Benedetti–Jesuit relationship (Wallace 1987b) and the second on influences on the Jesuits that derived from Domingo de Soto (Wallace 1987a). Neither of these studies, on the one hand, is readily available; both, on the other, are heavily documented with references to source materials and to Latin texts. Since readers of this journal are not primarily interested in history, I have pruned much of the documentation from my presentation here. However, to make available a detailed justification of my thesis for those who might be interested, I have included parenthetical references to the footnotes of the two studies in the body of the text.

[2] The numbers here and following continue the page enumeration of the previous citation in the text.

[3] Preparation of this essay was supported in part by a grant from the National Endowment for the Humanities, an independent agency of the U.S. government.

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VI

Quantification in Sixteenth-Century Natural Philosophy

The thesis of this essay is that the scientific revolution of the seventeenth century grew directly out of progress made in the study of natural philosophy during the sixteenth century, particularly through the introduction of new techniques in quantification. This development had its beginnings at Oxford and Paris in the fourteenth and fifteenth centuries, but it reached its culmination in the Iberian peninsula and in Italy during the sixteenth century. It was in these regions that the obstacle presented by *metabasis,* that is, the use of mathematical principles in the study of nature, was successfully overcome.[1] Once *metabasis* ceased to be an obstacle, the way was prepared for a successful merging of the tradition of natural philosophy, which provided the main core of teaching in the universities, with the "quadrivial" tradition (including optics and Archimedean statics), then regarded as a necessary propaedeutic for university studies though playing a minor role in the curriculum. The main arena for this development was the study of motion and of force as the source of motion and of rest, although the study of light was also of some importance. In what follows I give most attention to the quantification of space, of motion, and of force; after this I offer briefer comments on the quantification of light as relevant to my thesis. I conclude with a more general reflection on the relation of the Aristotelian-Thomistic tradition to recent philosophy of science.

THE QUANTIFICATION OF SPACE

Let me begin with the quantification of space. This would seem to be the least controversial, for Aristotle himself discussed space in the fourth book of his *Physics* and then dealt extensively with its quantitative parts in the sixth book, with his analysis of the continuum. Rather than focus on space, however, I would remark on Aristotle's definition of place, particularly as this was expounded by the sixteenth-century Dominican commentator, Domingo de Soto. Soto is a key figure in my thesis, for early in that century he had studied

at the University of Paris and there learned all of the "calculatory" techniques worked out by the Oxford Mertonians and the *Doctores Parisienses* during the fourteenth and fifteenth centuries. Instead of using such techniques in solving *sophismata* or in exercises conceived only *secundum imaginationem,* as had been the fashion at Oxford, Soto applied them to problems in the real world. He once stated that he began his intellectual life at Paris among the nominalists, but that he was reared in Spain, as a Thomist, among the realists. Perhaps this explains why he spent most of his life working out the tensions between the nominalists and the realists, and in so doing made a significant contribution towards the development of a mathematical physics in the latter part of the century.[2]

Aristotle, as is well known, defined the place of an object as "the innermost motionless surface" of the medium containing it (212a 20–21). The problem this definition presents is one of understanding precisely what Aristotle meant by the term "motionless," since any physical medium can be, and usually is, in motion at any particular time. Soto exemplifies this with the air in his classroom, or the air surrounding Salamanca; these seem to change continually, although the place of the room and the city apparently do not.[3] His answer to the difficulty is that the "innermost surface" of the containing medium is not to be taken materially, as a surface of air or water or other matter, but rather formally, as a physical surface implying a relationship to the center of gravity and to the poles and the ultimate sphere of the universe. With such a frame of reference, the place of Salamanca can be specified in terms of latitude and longitude regardless of the particular medium surrounding it, and even were the city to change its place, say, in an earthquake, the new place, whatever the surrounding medium, would be "motionless" in the same reference frame. Essentially Soto interprets Aristotle's definition in terms of a system of polar coordinates, and this enables him to speak meaningfully about motion in a void between the sphere of the moon and the earth's surface, were God to annihilate the intervening matter, and so to identify positions throughout the entire universe.

The point may seem trivial, but let me jump some decades ahead to Rome, where the German Jesuit Christopher Clavius was teaching mathematics at the Collegio Romano.[4] Clavius had studied at Coimbra under Pedro Nuñez, and one of his colleagues was the Spaniard Francisco Toledo, who had been Soto's "favored disciple" at Salamanca. Both were imbued with the realist spirit then abroad on the Iberian peninsula, which encouraged them to take mathematics seriously when applied to nature. Well known is Clavius's interest in the physical reality of eccentrics or epicycles; perhaps less well known is his own determination of the place of the nova of 1572 beyond the sphere

of the moon, and the implications he saw for this in understanding, and correcting, the *De caelo* of Aristotle, especially in its teaching on the incorruptibility of the heavens.

At this time controversy was abroad in Italy over the certitude of the mathematical sciences. Alessandro Piccolomini, who took issue with the mathematicians, denied that their disciplines could even be regarded as sciences in the strict sense of Aristotle's *Posterior Analytics*.[5] A few Jesuit philosophers at the Collegio, and Benito Pereira was one, embraced the anti-mathematical side in the controversy. In effect Pereira revived the *metabasis* prohibition of earlier centuries and attempted to proscribe the use of mathematical argument in natural philosophy. Clavius successfully overcame Pereira's opposition and influenced a series of young Jesuit professors at the Collegio—Antonius Menu, Paulus Vallius, Muzio Vitelleschi, and Ludovicus Rugerius—to incorporate his mathematical demonstrations into their treatises on the *Physics* and the *De caelo*. This they did, even though it enormously complicated their analyses of the alterability and the corruptibility of the heavens. The young Galileo, as I have argued elsewhere, appropriated their teaching notes, with interesting consequences, of which more later.[6]

Part of the Italian controversy over the mathematical sciences was about the validity of what I shall call "positional mechanics," the Renaissance treatment of statical problems similar to Jordanus Nemorarius's *Scientia de ponderibus*, though worked out more thoroughly in the tradition of Archimedes and the *Quaestiones mechanicae* of the Aristotelian school. Clavius left no treatises on this subject, but one of his students, Giuseppe Biancani, S.J., did. Biancani later taught at Parma, where the famous astronomer Giambattista Riccioli, S.J., was in turn his student. The treatises written by Biancani on the mathematical and the physico-mathematical sciences justify them as conforming to the norms of the *Posterior Analytics*.[7] Not only that, but commentaries on the *Posterior Analytics* by Vallius, Vitelleschi, and Rugerius expand greatly the space allocated to the foreknowledge required for demonstration, devoting particular attention to the *suppositiones* employed in the "mixed sciences" and how they come to be certified.[8] The young Galileo was acquainted with these commentaries also, and indeed copied out Vallius's lectures on *De praecognitionibus* and *De demonstratione* in his little-known logical treatises, which I published only in the last decade.[9]

THE QUANTIFICATION OF MOTION

Positional astronomy and positional mechanics, based as they were on the quantification of physical space, advanced considerably in the sixteenth century. They were merely propaedeutic, however, to an even greater advance in

the quantification of motion. The fourteenth century had seen important beginnings in this area with the introduction of the "mean-speed theorem" and various calculatory treatises on the ratios of motions. As far as we know, none of these progressed substantially beyond the status of imaginative mathematical exercises. At sixteenth-century Paris, however, a definite change took place. Natural philosophers there—John Major, John Dullaert of Ghent, Juan de Celaya, to name a few—incorporated these techniques of quantification into their commentaries and "questionaries" on Aristotle's *Physics*.[10] Domingo de Soto had studied under Celaya at Paris and brought these analytical methods back to Spain, and eventually to Salamanca, in his own teaching of natural philosophy. Soto is justly famous for being the first, as far as is known, to apply correctly the expression *uniformiter difformis* to falling motion.[11] Around 1550, in his questions on the *Physics*, he described the free fall of a heavy body as uniformly accelerated, and gave figures showing that he interpreted this as velocity increasing in direct proportion to the time of fall.[12]

There has been some question as to how widely diffused was Soto's teaching later in the sixteenth century. His *Physics* was reprinted seven times in Spain and Italy, and later in Douay, into the early seventeenth century, and thus was available for anyone to read. More significant, in my view, is the fact that his analysis of free fall was taken up and developed in a number of manuscripts by Jesuits at Coimbra and at Evora, in Portugal, and also cited approvingly by Rugerius at the Collegio Romano.[13] The teaching, therefore, was very much alive. The problem is whether or not it was ever experimentally verified, and how it came to be passed on to Galileo, if indeed it was.

Before coming to that, I would call attention to another development relating to the quantitative description of motion: the difference between rectilinear and circular motion and how this might be applied to the natural and violent motions of elemental bodies. With regard to rectilinear motion, the downward motion of a heavy body, i.e., that toward the center of gravity, would be natural, whereas its upward motion, that away from the center, would be violent. For fire, the lightest body, the opposite would be true, but here a more interesting case presents itself. As fire recedes naturally from the center of gravity toward the sphere of the moon, it ultimately comes to that sphere, and then can only move in a circle. This was not thought to be unusual by Aristotelians, for fire would then seem to be taking on a motion *praeter naturam,* one above its nature, yet similar to that of the heavenly bodies. Albert of Saxony even speculated that as fire approached the lunar sphere its motion would become composite, involving both a rectilinear natural motion and a circular preternatural motion, thereby adumbrating the "superposition theorem" of modern mechanics.

But the key question is this: granted that the motion of fire at the sphere of the moon is circular, is such a motion natural or violent? The question has a long history with Greek and Arab commentators, and among the Latins was discussed by Albertus Magnus,[14] Thomas Aquinas, Albert of Saxony, and Nicole Oresme. In northern Italy it occupied the attention of Paul of Venice and Pietro d'Abano, and then, in the sixteenth century, was discussed by Marcantonio Zimara, Jacopo Zabarella, and the Jesuits of the Collegio Romano. The general answer that emerged was that the circular motion of fire (and indeed of any element, whether at the sphere of the moon or not), is neither natural nor violent, but is actually a third type intermediate between the two, variously identified as neutral, indifferent, or middle motion. The Jesuit Vitelleschi even speculated that such *motus medius,* once started in a resistanceless medium, would be uniform and perpetual, since nature would neither increase it nor diminish it, so long as it maintained the same distance from the center of gravity. This comes close to the idea of "circular inertia," adumbrated in Galileo's early *De motu* manuscripts and long thought to have been completely original with him. What is remarkable is that Galileo's notes on *De caelo,* based on these Jesuit lectures, reveal a prior knowledge of this neutral motion, even apart from his reference to it in the *De motu antiquiora.*[15]

THE QUANTIFICATION OF FORCE

Having introduced the notion of nature acting to increase or diminish a motion, I pass from the quantification of space and of motion to the next consideration, the quantification of force. In positional mechanics, static forces had been quantified from the time of Archimedes onward, and they had received fuller development in the Middle Ages with the work of Jordanus and others. Here I wish to consider dynamic forces, those associated with motion, in the sense employed by Aristotle in the fourth and seventh books of his *Physics.* These had been considered in fourteenth-century Oxford and Paris, but mainly *secundum imaginationem* and without specific reference to real forces operative in the world of nature.

Here too Domingo de Soto provides a starting point for the sixteenth-century development. The setting in which he worked was different from that of the fourteenth century, for by this time John Philoponus's commentary on the *Physics* had become available in the Latin West. In this commentary Philoponus introduced the notion that nature itself was a *vis* or force operative from within a substance and causing it to have its characteristic activities.[16] Now Soto had adopted the nominalist solution to the projectile problem, which taught that the projectile was not moved by the surrounding medium

(as Aristotle himself had held), but rather by an impetus placed in the projectile by an external agent, the projector. Thomists at the time were divided on Aquinas's teaching in this matter, for in some texts St. Thomas seems to employ impetus whereas in others he seems to reject it. Soto favored the former interpretation, attributing the impetus doctrine to Aquinas, and then explained it in the following way. Impetus was like a force impressed on the projectile, which, after it left the projector, moved the projectile from within in much the same way as the *gravitas,* or *vis gravitatis,* moves the heavy object from within during its fall towards the center of gravity.[17] During the fall of the object, Soto held, its natural gravity remains the same, but its accidental gravity, namely, that acquired through the impetus of its fall, gradually builds up, and this serves to explain why the body accelerates as it approaches the center of gravity.[18] Thus force came to be, for Soto, a key causal factor in explaining why bodies move the way they do in both natural and forced motion.

These ideas were assimilated by the Roman Jesuits, as I have pointed out elsewhere, for one can find references to motive forces, resistive forces, and even occult forces in their notes on the motion of heavy bodies.[19] They offered a variety of teachings on the nature of impetus or *virtus impressa,* and on how this type of force can be used to explain phenomena not otherwise explicable with Aristotelian principles. When accounting for velocity increase with time of fall, surely a difficult problem, they combined motive and resistive forces in a distinctive way. Vitelleschi, for example, rejected the explanation offered by Galileo in his *De motu antiquiora,* where the Pisan physicist held that the increase occurs only at the beginning of the motion because there it gradually overcomes a residual lightness, a *privatio gravitatis,* left in the body by reason of its position. One might regard this as a type of internal resistance that has to be overcome before the falling body can reach its terminal velocity. Following Zabarella, Vitelleschi invokes an external resistive force as well. In his view, the falling body impels the medium in such a way that it offers less and less resistance to the body's motion; thus, the farther the body falls the faster it moves. This comes about, in his account as in Zabarella's, because the velocity of any motion results from an excess of the motive force over the resistance encountered; therefore the velocity of fall increases as the *vis gravitatis* builds up and the resistance grows less.

Such quantification of force, admittedly proposed only in a general way, found echo in a contemporary development in northern Italy, that of the Venetian mathematician and physicist Giovan Battista Benedetti, who is generally thought to have influenced Galileo's early writings on motion.[20] Like Soto and late sixteenth-century Jesuits, Benedetti used internal forces to ac-

count for projectile and gravitational phenomena, and combined them in ways that suggest a knowledge of the superposition theorem. He argued, as did Galileo, that velocity of fall is dependent not on weight but on specific gravity, and that all bodies will fall with the same speed *in vacuo,* where buoyancy and resistive effects can be neglected. Gravity and levity became for Benedetti relative concepts, so that for him, as for Archimedes and the Jesuit Vallius, air has no weight in air, nor water in water. And he analyzed the case of a body falling through the center of the earth to conclude that it would oscillate about the center, on the analogy of the motion of the bob of a pendulum of exceedingly long length.

These discussions of falling bodies are of interest for the proportionalities they suggest between motive and resistive forces, but they do not address the more important problem of measurement and experimental confirmation. Here I return again to Domingo de Soto to bring out a point I have made in a study of Benedetti. Benedetti's father was a Spaniard, and Benedetti himself was the friend of a Spanish Dominican, Petrus Arches, whom he praises lavishly in his *Resolutio* of 1553 and his *Demonstratio proportionum motuum localium contra Aristotelem et omnes philosophos* of 1554. It was Arches, in fact, who told Benedetti that criticisms of Aristotle's dynamic laws were being discussed in Rome the summer before Benedetti prepared his *Demonstratio.* Perhaps Arches knew of a work then just published at Brescia, in which the author, Giovanni Battista Bellasco, inquired why a ball of iron and one of wood fall to the ground at the same time. Soto himself does not give experimental support for his *uniformiter difformis* doctrine, but it is noteworthy that around the time of his writing there was some discussion of empirical evidence. As early as 1544 tests were being performed to show that Aristotle was wrong in his claim that heavy bodies will fall to the ground at speeds directly proportional to their weights. Benedetto Varchi, in his *Questioni sull'Alchimia,* finished by that date, states that the Dominican Francesco Beato, a philosopher at Pisa, had disproved Aristotle's claim. Shortly thereafter, and before finishing his *Physics* "questionary," Soto himself was in northern Italy attending the Council of Trent. The fact that he, Beato, and Arches were all Dominicans enhances the possibility of their sharing knowledge of these results.[21] And, of course, in 1575, Girolamo Borro, who was later to teach Galileo at Pisa, had stated that his experiments showed that a piece of wood reached the ground before a piece of lead, when both were projected from a second-story window. Only a year later, at Padua, Galileo's predecessor there, Giuseppi Moletti, reported a test in which a lead ball and a wooden ball, both of the same size, were released from a height and seen to reach the ground at

the same moment of time. Very rough measurements, one might say, yet sufficient to refute Aristotle as an authority in the matter of free fall, and to set the parameters for further research.

More important, in my view, was the discussion of the *suppositiones* one must employ to have demonstrations in a mathematical physics based on such experimental findings. These are touched on in the Jesuit logic notes,[22] but they are much further developed by Benedetti and Galileo in the context of their treatment of impediments presented by the earth's geometry and various resistive effects. To my knowledge Benedetti does not discuss neutral motion, i.e., that intermediate between natural and violent motion, but for him horizontal motion for limited distances on the earth's surface would answer to that description. His basic thesis was that the only truly natural motion must be circular, for this alone can be perpetual. But, he reasons, there is no noteworthy difference between a perfect sphere and a plane surface of small extent. This explains why one will encounter no difficulty in moving a sphere along a horizontal surface; indeed, it can be moved by "a force no matter how small." In another context he qualifies an argument to specify that it holds only "when all impediment is removed." These techniques are remarkably similar to Galileo's, as I have abundantly documented in my *Galileo and His Sources.*[23] The transition from impetus theory to the principle of inertia came about in Galileo's later writings, where he could regard motions in a circle of large radius or over short distances as rectilinear, and could state that they could be initiated and maintained by "a force smaller than any given force." Effectively Galileo was able to eliminate the horizontal *vis* in intermediate motion, maintain that a body moving uniformly in a horizontal direction would not be constrained either to increase or to decrease its velocity, and so would continue to move uniformly, thus anticipating what would later be called Newton's first law of motion.

THE QUANTIFICATION OF LIGHT

This leads to my final topic, the quantification of light. It is not my intention to deal with theories of vision, as these have been adequately treated in the literature.[24] I intend merely to consider light under the aspect of the intensification of forms, to show how Soto's expression *uniformiter difformis* came to. be applied by late sixteenth-century Jesuits to optical phenomena.

The first person of interest here is Andeas Eudaemon-Ioannes, S.J., who taught physics at the Collegio Romano in 1597–98, and who left treatises on action and passion and on the motion of projectiles, both written in the man-

ner of the *Calculatores*.[25] His treatise on action and passion generalizes the expression *uniformiter difformis* so that it can be applied to all cases of physical agency. Aware that this is a work in mathematical physics, he first notes that some propositions from his physical disputations will be presupposed, and that here his immediate objective is to note and prove propositions pertaining to mathematics. His first *suppositio* is the key one: every natural agent acts *uniformiter difformis* on a quantified subject when applied to it. The justification for this is that physicists commonly concede it, because one sees that an agent when close acts more vehemently and when farther away less so; therefore, the closer the greater, the farther the lesser; therefore, as distance increases, action decreases; therefore the action itself is *uniformiter difformis*.

We need not enter into the mathematical details of the thirty-one propositions Eudaemon deduces from this, some with a substantial number of corollaries. Suffice it to mention that he retained his interest in impetus theory after leaving Rome and, while at Padua in the first decade of the seventeenth century, discussed the famous "ship's mast" experiment with Galileo. It was to this "Father Andreas" that Galileo confided that he actually performed the experiment, although he later denied that he had done so in the *Dialogue* of 1632 on the grounds that he did not have to experiment to be assured of its truth.[26]

I shall finally mention the work of Franciscus Aquilonis, a Belgian Jesuit who had completed his theology studies at Salamanca in the late 1590s.[27] Clavius had written a large number of treatises in applied mathematics but had never treated optics. Aquilonis set for himself the task of completing Clavius's *opera*, and out of this effort emerged his *Opticorum libri sex*, published at Antwerp in 1613. The fifth book of this work, dealing with the propagation of light, is written in the calculatory manner of Eudaemon's treatises, and shows the precise way in which light diminishes "uniformly difformly" as it is propagated from its source. This is the beginning of an important Jesuit tradition in optics, culminating in the *Physico-mathesis de lumine* of Francesco Mario Grimaldi, S.J., published in 1665 and now regarded as a key work in the modern development of that science.

AN ARISTOTELIAN-THOMISTIC POSTSCRIPT

What I have given is a quick overview of quantification in sixteenth-century natural philosophy, with main attention to motion and briefer notes on light. By the end of the sixteenth century there seems little doubt that methodological canons and conceptual apparatus for a mathematical physics of motion

and of light were already at hand. It remained, of course, for the mature Galileo, plus Kepler, Newton, and others to develop these ideas into the "new sciences" of mechanics and optics that came to flourish in seventeenth-century Europe. Scholars working in this later period of the scientific revolution frequently express doubt about its indebtedness to the sixteenth century. If one knows nothing about that century, surely it is easy to pass it over in silence. But for those acquainted with it, it can be seen and studied for what it truly was: a period of transition from the medieval to the early modern, one of singular value for the history and philosophy of science.

Another point should also be made. This is that much of what has just been presented emerged from the Aristotelian-Thomistic tradition, a tradition that historians and philosophers of science have much maligned in the past. Soto, of course, represents the Thomistic tradition at its best, and the Jesuits mentioned above, who worked mainly in the first decades of the society's existence, took seriously St. Ignatius's injunction to follow the teachings of both Aristotle and St. Thomas Aquinas. One could argue that their work, linked as it was to the methodological advances at Padua preserved in Zabarella's logical treatises, marked the zenith of that tradition's development—soon to come under attack and shunted aside, unfortunately, by the Jesuits' most famous student, René Descartes.

What was important about these sixteenth-century thinkers is that they recognized that the study of quantity pertains not only to mathematics but to natural philosophy and metaphysics as well. Quantity is an integral part of a thing's nature, and important for the fact that physical quantity is the intermediate through which the powers and sensible qualities proper to corporeal natures come to exist within their structures. In the sixteenth century little was known in detail about the powers that are proper to natural kinds—inorganic, plant, animal, and human. This was particularly true of the inorganic, though a start had been made on the *vis gravitatis,* the power or force of gravity. The application of mathematical reasoning to that power was Newton's supreme achievement and pointed the way to quantitative principles that would underlie the modern sciences of physics, astronomy, chemistry, geology, and ultimately molecular biology. In my *The Modeling of Nature* I have sketched the tortuous path that led from the power of gravity to the four forces of high-energy physics that ground all recent speculation about our cosmos, and then to the higher powers whereby these are related to the plant and animal kingdoms.[28] I have also sketched the major demonstrations that enabled us to progress from knowing mountains on the moon to knowing the

DNA molecule.[29] The middle terms of these demonstrations are in all cases quantitative, that is, based on physical quantity, but manipulated by mathematical reasoning to yield a result that is true in the world of nature.

The importance of the mathematical premises in this type of reasoning cannot be overstated. In the demonstrative regress (*regressus*), as developed by Zabarella and employed with consummate skill by Galileo, the same middle term appears in both the mathematical and the physical premise of each demonstration.[30] What is essential to the regressive process is showing that the middle term is convertible with the predicate in at least one of the premises. Aristotle's own example of how this is done is the proof of the moon's being a sphere from its having phases (*Posterior Analytics,* I, 13). That a spherical shape *alone,* of all possible solid surfaces that are externally illuminated, is able to produce the appearances of crescent, half, gibbous, and full phases may not be readily *seen* by the naturalist, but it can be quickly *demonstrated* by the mathematician in a branch of study known as projective geometry. Quantitative convertibility is far easier to see than convertibility in any other category. And it is precisely that type of convertibility that is mánifested in the key demonstrations sketched in *The Modeling of Nature.*

Four hundred years ago these demonstrations, had they been available, *per impossibile,* to natural philosophers, would have been understood by them and provided the basis for substantial restructuring of the special books of Aristotle's *libri naturales.* In the present day, especially when Aristotle's foundational treatises, the *Physics* and *De anima,* are largely *terra incognita* to philosophers, one would be rash to think of such restructuring as readily feasible. The situation is exacerbated by the fact that scientists have been brainwashed for centuries to picture the world in Cartesian coordinates, to see it as quality-less, power-less, and nature-less, extended as far as one wishes in *n*-dimensions, not one of which differs essentially from any other. But the challenge of restoring four centuries of forgetfulness of this once fruitful tradition is now there. It has to be faced by anyone who would bring both philosophy and science back to the world of nature and to all the wonders it presents to the modern mind.

NOTES

1. For an explanation of this concept, see Steven J. Livesey, "*Metabasis:* The Interrelationship of the Sciences in Antiquity and the Middle Ages," Ph.D. dissertation, The University of California, Los Angeles, 1982. See also his "The Oxford Calculators,

Quantification and Qualities, and Aristotle's Prohibition of *metabasis*," *Vivarium* 24 (1986), 50–69.

* 2. Details are given in my "Domingo de Soto and the Iberian Roots of Galileo's Science," in *Hispanic Philosophy in the Age of Discovery,* ed. Kevin White, Studies in Philosophy and the History of Philosophy, 19 (Washington, D.C.: The Catholic University of America Press, 1997), 113–29. For introductory materials relating to Soto's commentary and questions on the *Physics* of Aristotle, see my "Domingo de Soto's 'Laws' of Motion: Text and Context," in *Texts and Contexts in Ancient and Medieval Science: Studies on the Occasion of John E. Murdoch's Seventieth Birthday,* ed. Edith Sylla and Michael McVaugh (Leiden: E. J. Brill, 1997), 271–304.

3. In his first question on Book IV of Aristotle's *Physics,* fols. 58v–61r of the 1555 edition.

4. A recent study that supplies basic information about Clavius is James M. Lattis's *Between Copernicus and Galileo: Christoph Clavius and the Collapse of Ptolemaic Astronomy* (Chicago and London: University of Chicago Press, 1994).

5. For the definitive study of this controversy, see Anna De Pace, *Le Matematiche e il Mondo: Ricerche su un dibattito in Italia nella seconda metà del Cinquecento* (Milan: FrancoAngeli, 1993), esp. 21–75.

6. The main conclusions are given in my *Galileo and His Sources: The Heritage of the Collegio Romano in Galileo's Science* (Princeton: Princeton University Press, 1984). Details of some of the appropriated texts will be found in my *Galileo's Early Notebooks: The Physical Questions: A Translation from the Latin, with Historical and Paleographical Commentary* (Notre Dame: University of Notre Dame Press, 1977).

7. *Galileo and His Sources,* esp. 126–48 and 202–16.

8. This development is described in my "Aristotle and Galileo: The Uses of *Hupothesis* (*Suppositio*) in Scientific Reasoning," in *Studies in Aristotle,* ed. D. J. O'Meara, Studies in Philosophy and the History of Philosophy, 9 (Washington, D.C.: The Catholic University of America Press, 1981), 47–77. This article has been reprinted as Essay 3 in my *Galileo, the Jesuits and the Medieval Aristotle,* Collected Studies Series. CS346 (Aldershot, U.K.: Variorum Publishing, 1991).

9. The Latin text is given in Galileo Galilei, *Tractatio de praecognitionibus et praecognitis* and *Tractatio de demonstratione,* transcribed from the Latin autograph by W. F. Edwards, with an introduction, notes, and commentary by W. A. Wallace (Padua: Editrice Antenore, 1988). This is translated in my *Galileo's Logical Questions: A Translation, with Notes and Commentary, of His Appropriated Latin Questions on Aristotle's* Posterior Analytics, Boston Studies in the Philosophy of Science, 138 (Dordrecht-Boston-London: Kluwer Academic Publishers, 1992).

10. I have sketched various aspects of this development in my *Prelude to Galileo: Essays on Medieval and Sixteenth-Century Sources of Galileo's Thought,* Boston Studies in the Philosophy of Science, 62 (Dordrecht-Boston: D. Reidel Publishing Company, 1981).

* 11. Details are given in my "The Enigma of Domingo de Soto: *Uniformiter difformis* and Falling Bodies in Late Medieval Physics," *Isis* 59 (1968), 384–401, reprinted in my *Prelude to Galileo* (note 10 above), 91–109.

12. Additional details relating to Soto's calculations will be found in my "Domingo de Soto's 'Laws' of Motion: Text and Context" (note 2 above).

* 13. The Portuguese manuscripts are discussed in my "Late Sixteenth-Century Por-

tuguese Manuscripts Relating to Galileo's Early Notebooks," *Revista Portuguesa de Filosofia* 51 (1995), 677–98. They are also analyzed in the essay cited in the previous note.

14. Albert the Great seems to have initiated treatments of this problem in the Latin West. See my "Galileo's Citations of Albert the Great," *Albert the Great: Commemorative Essays,* eds. F. J. Kovach and R. W. Shahan (Norman: University of Oklahoma Press, 1980), 261–83; reprinted in *Prelude to Galileo* (note 10 above), 264–85.

15. See *Prelude to Galileo,* 270–74, with the additional references cited therein.

16. Philoponus's teaching on nature is sketched in Essay 13 of *Prelude to Galileo* (note 10 above), entitled "Galileo and the Causality of Nature"; see p. 290.

17. In his questions on the *Physics* of Aristotle, Bk. 8, quest. 3. This teaching and that of other Thomists on the projectile problem are discussed by James A. Weisheipl in his *Nature and Motion in the Middle Ages,* ed. William E. Carroll, Studies in Philosophy and the History of Philosophy, 11 (Washington, D.C.: Catholic University of America Press, 1985), 68–69.

18. This teaching is touched on by Soto in his commentary on the *Physics* of Aristotle, Bk. 8, chap. 9, text 76, and by Jesuit commentators who appropriated his teachings; see my "Domingo de Soto's 'Laws' of Motion: Text and Context" (note 2 above), 303–4.

19. See my "Causes and Forces in Sixteenth-Century Physics," *Isis* 69 (1978), 400–412, reprinted in *Prelude to Galileo,* 110–26; also *Galileo and His Sources* (note 6 above), 191–202.

20. For Benedetti's views, see my "Science and Philosophy at the Collegio Romano in the Time of Benedetti," in *Cultura, Scienze et Techniche nella Venezia del Cinquecento,* Atti del Convegno Internazionale di Studio "G. B. Benedetti e il suo tempo" (Venice: Istituto Veneto di Scienze, Lettere ed Arti, 1987), 113–26; reprinted as Essay 8 in *Galileo, the Jesuits, and the Medieval Aristotle* (note 8 above).

21. For more particulars, see notes 66 and 67 to the essay cited in the previous note.

22. For the Jesuit teachings appropriated by Galileo, see *Galileo and His Sources* (note 6 above), 112–14; also "Aristotle and Galileo: The Uses of *Hupothesis* (*Suppositio*) in Scientific Reasoning" (note 8 above).

23. See pp. 230–54.

24. See David C. Lindberg, *Theories of Vision from al-Kindi to Kepler* (Chicago: University of Chicago Press, 1976).

25. On Eudaemon's "calculatory" treatises, see my "The Early Jesuits and the Heritage of Domingo de Soto," *History and Technology* 4 (1987), 301–20, esp. 306–8, reprinted as Essay 6 in *Galileo, the Jesuits, and the Medieval Aristotle* (note 8 above).

26. See *Galileo and His Sources* (note 6 above), 269–71.

27. The optical work of Aquilonis, better known as François de Aguilon, is sketched in "The Early Jesuits and the Heritage of Domingo de Soto" (note 23), 311–12. He is the subject of a monograph by August Ziggelaar, S.J., *François de Aguilon, S.J. (1567–1617)* (Rome: Institutum Historicum S.I., 1983).

28. This in the first five chapters. The full title of the work is *The Modeling of Nature: Philosophy of Science and Philosophy of Nature in Synthesis* (Washington, D.C.: Catholic University of America Press, 1996).

29. In chapters 9 and 10, after the necessary logical apparatus has been set up in chapters 7 and 8.

* 30. A concise explanation of the regress is given in *The Modeling of Nature,* 300–308. For a full account of the method in the history of science, see my "Galileo's Regressive Methodology: Its Prelude and Its Sequel" in *Method and Order in Renaissance Philosophy of Nature: The Aristotle Commentary Tradition,* ed. D. A. Lisca, Ekhard Kessler, and Charlotte Methuen (Aldershot, U.K.: Ashgate, 1997), 229–52. Also relevant is my "Circularity and the Demonstrative *Regressus:* From Pietro d'Abano to Galileo Galilei," *Vivarium* 33.1 (1995), 76–97.

THE CERTITUDE OF SCIENCE IN LATE MEDIEVAL AND RENAISSANCE THOUGHT

IN a recent study entitled *Probability and Certainty in Seventeenth-Century England*, Barbara Shapiro points to the erosion of the traditional Aristotelian concepts of science and certitude as giving new direction to the work of English intellectuals in the seventeenth century.[1] Without denying that such an identifiable change may have taken place there and then, in this essay I would like to sketch a broader panorama of changing attitudes toward the certitude of science—focusing on Western Europe from the thirteenth to the seventeenth century. And rather than analyze the work of one scientist or of a small group, I shall draw on a number of themes in my previous writings to accent the complexity of this issue when addressed over four centuries of scientists and scientific growth. It is difficult to make generalizations over such a long period, and yet I shall argue that discernible patterns emerge. Put somewhat schematically, the period prior to the Condemnation of 1277 saw the greatest confidence in the certitude of science; after that, the rise of nominalism led to an erosion of that confidence, terminating in the eclectic pluralism of the late fifteenth and early sixteenth centuries; the Renaissance and "Second Scholasticism" contributed to such erosion with disputes over disciplinary domains between philosophers and mathematicians and the probabilist controversies of theologians; but Galileo reasserted that confidence with the demonstrations of his "new science," which were to inspire not only Descartes and Newton, but a host of scientists down to John Herschel in the nineteenth century. So my thesis is that certitude was not seriously claimed for natural science during the late Middle Ages and the early Renaissance, but that it began to be claimed again in Italy in the early seventeenth century, precisely when Shapiro says it was being rejected in England.

I. Science In The High Middle Ages

The ideals of *scientia*, or certain knowledge reasoned to from evident first principles by causal analysis, were well understood in the High Middle Ages. The recent studies of Steven Marrone on William of Auvergne and Robert Grosseteste and of David Lindberg on Roger Bacon, plus earlier works on Grosseteste, Albertus Magnus, and Thomas

Aquinas, portray how these investigators rediscovered Aristotle's *Organon* and applied it assiduously to uncovering the secrets of nature.[2] All had confidence that truth and certitude could be attained in a general way using the Aristotelian canons, even though there were many matters—about the heavenly bodies, for example—on which doubts could be and were expressed.[3] Because of Grosseteste's and Bacon's concern with light, I should note the strong attraction exerted by mathematics in their optical treatises, so much so that one might say that these English writers pursued the ideal of a "mixed science" (or a *scientia media* intermediate between mathematics and physics) rather than a "pure physics" in the Aristotelian sense. Albertus Magnus, on the other hand, in attempting to develop all the sciences contained in the *libri naturales* of the Arabs as well as in Aristotle's writings, saw more of the difficulty involved in natural science as such. Indeed, he cites several times a statement of Ptolemy to the effect that naturalists always disagree over their science and are not like mathematicians in this regard, so that perhaps mathematics alone can lay claim to being a strict science in the sense of attaining certitude.[4] The use of eccentrics and epicycles to account for the heavenly motions was one of the matters on which Albertus, like his student Aquinas, had doubts. And yet, as I have argued elsewhere, Albertus also analyzed carefully the suppositional necessity that characterizes nature's operations, and explained how one might have strict demonstrations, in the Aristotelian sense, if one could formulate a demonstration *ex hupotheseōs*, as Aristotle himself puts it, or *ex suppositione finis*, in the Latin terminology of Albertus and Aquinas.[5]

It was Aquinas who best explained this suppositional methodology in his commentaries on the *Physics* and the *Posterior Analytics*.[6] The problem arises because the world of nature is contingent and defectible: nature acts for an end, but it does not achieve that end infallibly and invariably, mainly because its agencies work across time and with matter that can prove to be defective. If one observes the regularity of its operation, however, since it does achieve ends regularly and for the most part, one can reason *ex suppositione finis*, that is, from the supposition of an end being attained, to the antecedent causes that are necessary for its attainment. In this way causal analysis can yield truths about nature that are certain, even though these do not have the absolute necessity that is found in mathematics and metaphysics.

This doctrine on necessity and demonstration *ex suppositione* is not stressed in any of Averroës's commentaries, to my knowledge, and it does not seem to have been part of the intellectual equipment of the Latin Averroists.[7] It certainly was not appreciated by Etienne Tempier, who construed the natural philosophy of the Paris Aristotelians as a type of metaphysics that was incompatible with the Catholic faith. He struck forcibly at the alleged truth and certainty of many theses of the Averroists concerning man and the physical universe in the famous Condemnations of 1270 and 1277.[8] As is well known, Aquinas (and Albertus implicitly)

came under a shadow at Paris and Oxford as a result of the condemnations, and the Franciscans quickly challenged the Dominicans on the grounds of their orthodoxy at those centers of learning.

II. Late Medieval Science

Let us take 1277, then, as the point of demarcation for the Late Middle Ages. The characteristic note in philosophy and theology for that period, as found in both Scotus and Ockham (though in different ways), was the accent on will as opposed to intellect, which led to a predominance of voluntarism over the intellectualism that had hitherto prevailed. For those interested in late medieval science, Ockham exerted the definitive influence, and so his views merit brief examination. As he saw it, reality itself is a collection of absolute singulars, the distinguishable units of which are substances and qualities. Such singulars depend for their being on the will of God, and the will of God can accomplish anything that does not imply a contradiction. This being so, it is always possible to have one singular without the other. Since an effect is different from its cause, it is likewise possible for God to sustain the effect without its proper cause. The theory of divine omnipotence based on what God can will without contradiction therefore implied a universe radically contingent on the divine will, even to the natures of things themselves. And man's knowledge of that universe, in that nominalist perspective, could never be more than a *de facto* association of many singulars. This led inevitably to the questioning of the validity of causal reasoning, and thus of the certitude of demonstration in the Aristotelian mode. In a universe where there is no necessity, and where relations have no reality independent of things themselves, it becomes impossible to establish the truth of causal propositions. Although Ockham wrote a treatise *De demonstratione*, he did not conceive demonstration as an apodeictic form of reasoning. In the sciences of nature, as a consequence, the best one could hope for would be a highly probable proposition.[9]

In England, where Ockham's ideas early took root, "that uncertain feeling" became quite pervasive. Logic, to be sure, flourished, and all the modes of *consequentiae* and of hypothetical reasoning were investigated in exhaustive detail. But natural philosophy would never yield a conclusion that could give the ecclesiastical authorities cause for alarm. *Sophismata* and *dubitabilia* became the stock in trade of those pursuing the science of nature. Working *secundum imaginationem*, investigators at Merton College, Oxford, explored many of the kinematic properties of moving bodies.[10] Yet the extent to which their analyses might be applied to the world of nature was never fully appreciated. Richard Swineshead's study of the place of an element, *De loco elementi*, and the various factors that would affect an extended body as its center approached, and then passed, the center of gravity of the universe, is typical in this regard. Indeed, it has been remarked that his calculations of elemental movement were made primarily to show that mathematical techniques are them-

selves inapplicable to the very motions they were designed to study.[11]

The nominalism that was pursued at the University of Paris in the fourteenth century owed much to Ockham and the Mertonians. Still, there are two important particulars in which its partisans, Jean Buridan and those associated with him, departed from Ockham and his *via moderna*. The first was in their understanding of motion and the causality involved in its production, and the second was in their estimation of the truth and certitude to be found in the science of nature.[12] Ockham, with a sweep of his mythical razor, had denied that motion was an absolute entity and thus taught that it was not a reality distinct from the body being moved. Since it could not be a new effect, to be consistent with his philosophy it would not require a cause.[13] Buridan rejected this line of reasoning, and so initiated a school of thought wherein the causes and effects of local motion were studied, and wherein medieval dynamics, with its doctrine of impetus and its analyses of the forces and resistances affecting the motions of bodies, received its highest state of development.

Even more important was Buridan's rejection of the Ockhamist attempt to invalidate a science of nature on the basis of "cases that are supernaturally possible," namely, those invoked in the Condemnation of 1277. Here Buridan returned to the earlier teaching of Albertus Magnus and Aquinas and argued that truth and certitude are attainable in the study of natural things provided demonstrations are made there *ex suppositione*. The nuance added by Buridan is that such demonstrations presuppose an order of nature that has been willed by God, wherein regularity and order prevail, and wherein a natural truth and certitude are to be found.[14] Buridan's *ex suppositione naturae* may be seen as a refinement of Aquinas's *ex suppositione finis*, for it already is implicit in Aquinas's writings. Ernest Moody misread Buridan when he saw the expression *ex suppositione* in his writing and argued that after Buridan "an ineradicable element of hypothesis (was) introduced into the science of nature."[15] *Suppositio* and *hypothesis* admittedly mean the same thing, but Ockham used *hypothesis* to mean that scientific reasoning could achieve no more than probability, whereas Aquinas and Buridan invoked *suppositiones* precisely to safeguard the truth and certitude of science's conclusions.

It is difficult of assess the overall effect of Buridan's correctives to Ockham on subsequent commitments to certitude in late medieval science. From what I have read in Nicole Oresme, I sense that he had a sophisticated view of the problem, that he allowed for many possibilities of error and falsehood, but that overall he subscribed to Buridan's more Thomistic analysis rather than Ockham's. Perhaps, however, the tactics of Albert of Saxony are more representative of the ways in which later fourteenth-century thinkers got around the problem. In Albert's time the question whether local motion is something distinct from the object moved and from its place was much discussed, and it was customary for nominalists to answer it in the negative and realists in the affirmative. Marsilius of Inghen clearly took the nominalist stance, whereas Buridan

took the realist. When Albert came to take up the difficulty in his questions on the *Physics*, Book III, he straddled the fence in the following way. In Question 6, considering the problem logically, he concluded in favor of the nominalists, while in Question 7, wherein he further admitted "divine cases," i.e., those that are supernaturally possible, he concluded with the realists. Following logic alone, therefore, he turned out to be a nominalist, whereas "according to truth and to the faith" he professed himself a realist.[16]

The proliferation of treatises during the fourteenth and fifteenth centuries wherein authors argued theses both *secundum viam nominalium* and *secundum viam realium* must have had a disconcerting effect for proponents of the certitude of science. One might interpret it as a continuation of the Averroist notion of "double truth," but it could equally be regarded as a dilution of the very idea of truth and the substitution of alternative defensible opinions instead. As the fifteenth century wore on, with the invention of printing and the publication of the *opera omnia* of medieval doctors such as Aquinas and Scotus, the situation was exacerbated even more. Religious orders exerted their influence in the universities, and soon there were not only nominalist chairs but Thomistic and Scotistic chairs as well. With the beginning of the sixteenth century, Jean Mair's scholastic revival at the University of Paris reflected this eclectic and pluralist situation.[17] Augustinians and Franciscans and Dominicans vied with each other in the preparation of manuals. The secular master, Juan de Celaya, who was to exert a marked influence on Domingo de Soto, added a subtitle to most of his treatises, explaining that his questions were being presented *secundum triplicem viam: beati Thomae, realium, et nominalium.*[18] Others added to these the *realissimi* and the variations among the nominalists—the Ockhamists and the followers of Gregory of Rimini. Each school, to be sure, could see its distinctive positions as true and certain, but the overall impression was unmistakable. On many important issues in natural philosophy there was no universal agreement, and thus one could not be certain about any of the propositions being taught.

III. The Renaissance And Second Scholasticism

The dividing line between late medieval and Renaissance science is difficult to draw. With the rediscovery and publication of Greek commentaries on Aristotle, however, one could say that there was a rebirth of learning even in natural philosophy. This, unfortunately, only succeeded in adding another voice to the many already clamoring at the end of the Middle Ages, that, namely, of the peripatetics who took their truth straight from "the master of all who know." A more decisive influence came from the mathematicians, and particularly from the Polish astronomer Nicholas Copernicus. The Pythagorean alternative to a geocentric universe, and some of the simplifications it introduced into theories involving eccentrics and epicycles, diverted attention to

mathematics as a possible source of truth and certitude regarding the physical universe. Here the disputes between schools overflowed into a larger argument over disciplinary domains: who was better equipped to yield a certain conclusion about the heavens, the philosopher or the mathematician? (I pass over the theologian here, but everyone knew after 1277 that one did so at one's peril!) The conventional wisdom then was that the mathematical astronomer could do no more than "save the appearances"; it was the philosophical astronomer who would have to pass on the natures of the heavenly bodies and the physical causes of their motions.[19] Mathematical theories pertained at best to a *scientia media* and thus could be regarded only as *scientia secundum quid*, one incapable of generating certitude in the domain of physics.[20]

An interesting development then took place in the latter part of the sixteenth century, when peripatetics reacted against the mathematicism of the day in the person of Alessandro Piccolomini. Piccolomini and his disciples wrote a number of treatises on the certitude of the mathematical disciplines (*De certitudine mathematicarum disciplinarium*) in which they attacked not only the certitude of applied mathematics but that of pure mathematics as well.[21] Their contention was that all mathematics failed to meet the rigorous canons of Aristotle's *Posterior Analytics*, that it did not demonstrate strictly, that it had no knowledge of causes, and that its conclusions were therefore not certain. This discredited the mathematicians still more in Renaissance Italy, and perhaps explains why much of their work was done outside the universities rather than in collaboration with philosophers who shared their common concerns.

Having mentioned the theologians, let me now add a final complicating factor bearing on scientific certitude. The Protestant Reformation by this time was in full flower, and the Catholic Counter-Reformation had already begun its course. The scholastic revival initiated in Paris at the onset of the sixteenth century now flourished as Second Scholasticism in Italy and on the Iberian peninsula. Thomistic and Scotistic and nominalist rivalries were as pervasive in theology as they had been in philosophy, only now a more powerful faction was coming into power, the newly established Society of Jesus. Thomism had been endorsed by its founder, Ignatius Loyola, but soon that disintegrated into competing schools: Suarezianism, Molinism, Bañezianism.The *Congregatio de auxiliis* tried to mediate the disputes between the Dominicans and the Jesuits, and ended by allowing each order to teach its distinctive doctrines on grace and free will without accusing the other of heresy. Thus there had to be some latitude in the certitude accorded to the teachings of dogmatic theology. In moral theology the emerging problems were even more difficult. Probabilism was countenanced in many areas, and rigorous solutions given up in this most delicate field of Catholic teaching.[22]

All of this could not help but have some influence on the certitude to be expected in Renaissance science. The Jesuit professors at the Collegio Romano, on whose class notes Galileo drew for his own early Latin com-

positions, present an interesting case history in this regard.[23] Disciplinary domains were as jealously guarded at the Collegio as elsewhere, despite the fact that the mathematicians and the philosophers were all Jesuits, while at the same time both sides were aware of the probabilist reasonings of the theologians. The principal mathematician at the Collegio was Christopher Clavius, whose commentary on the *Sphere* of Sacrobosco was the main text used for teaching astronomy. This work first appeared in 1570 and was revised in 1581, after which there were many more editions and reprintings. Between 1570 and 1581 an important astronomical event occurred—the nova of 1572. Clavius studied the nova carefully, and on its basis introduced two changes into his text that are noteworthy for our purposes. First, after discussing the nova and its position in detail, Clavius remarks in the second edition that the peripatetics will now have to see how Aristotle's opinion on the matter of the heavens can be saved. He goes on to speculate that this is not a fifth essence but rather matter that is changeable, though not as readily changeable as that found in terrestrial bodies. Second, when discussing the order of the heavenly spheres in the 1570 edition, he states simply that the Ptolemaic order assigned by Sacrobosco is true (*verum*) and that none of the alternative orderings accord with observations. In the second edition he qualifies the statement to read that the Ptolemaic order is truer than the others (*veriorem*) and more in conformity with the findings of experienced astronomers.[24]

These statements are cautious, to be sure, but they indicate a changing attitude toward the Aristotelian cosmos that was quickly reflected in the lectures on the *De caelo* being given by the Jesuit philosophers at the Collegio. They too were aware of the nova of 1572 and admitted that its position beyond the sphere of the moon had been demonstrated.[25] What were the resulting implications for teachings on the incorruptibility of the heavens and the matter of which they were composed? Antonius Menu, who taught the *De caelo* late in the 1570's, wrote simply: "It seems more probable and according to truth that the heavens are incorruptible by nature, although the contrary does not lack probability because of the authority of its proponents..."[26] This is the peripatetic view, only stated now in degrees of probability rather than with certitude. Ludovicus Rugerius, who covered the same matter in 1591, gave a more nuanced response in three conclusions: (1) "It is not yet completely improbable that the heavens are generable and corruptible through mutual transformation with lower bodies"; (2) "Much more probable is it that the heavens are generable and corruptible, but only through substantial transformation with other celestial parts"; and (3) "It is most probable...that the heavens are ingenerable and incorruptible, though this cannot be positively demonstrated."[27] Note here the probabilist language of the theologians cutting into the certitude of a conclusion universally accepted by the peripatetics in the universities of northern Italy. Similar statements can be found in both Menu and Rugerius when discussing the matter of which the heavens are composed. Even on the question of impetus the opposition between the nominalists and the peripatetics was softened.

Menu's teaching is somewhat representative: it is probable that projectiles are moved by the media through which they pass, he wrote, but it is also probable, and indeed more probable, that projectiles are moved not only by the medium but also by some quality such as a *virtus impressa* that inheres in them. Good arguments could be offered on both sides, and so one did not need to claim certitude for the nominalists here, even though Menu found their position preferable to that of the peripatetics.[28]

A similar reserve characterizes Galileo's notes on the *De caelo*, which seem to be based on the lectures of a Jesuit who taught between Menu * and Rugerius—probably those of Paulus Valla, the most likely source also of Galileo's logical questions. On the corruptibility of the heavens Galileo has only two conclusions, though both are supported by elaborate arguments. The first is this: if we speak of the heavens according to nature (and he adds other qualifications also), it is probable that they are corruptible. The second then reads simply: it is more probable that the heavens are incorruptible by nature.[29] Analogous probabilities characterize the ways in which Galileo sees the intelligences to be related to the heavenly bodies of which they were thought to be the movers.[30]

With this mention of Galileo we come to the threshold of the seventeenth century and the origins of modern science. It goes without saying that Galileo continued in his later writings to question the certitude of the conclusions presented in Aristotle's *De caelo*. But up to the time of his discoveries with the telescope he really had no alternative to put in their place. When he wrote his *Letter to Christina* of 1615, however, he was already making claims for necessary demonstrations based on evident sense experience as support for the Copernican world system.[31] And the new science of motion he proposed in *Discorsi* of 1638 was one erected on the model of Euclid and Archimedes, wherein scientific certitude was claimed for all its demonstrated propositions.

How Galileo came to reassert the truth and certitude of his mathematical physics is more than I can explain in this essay. Elsewhere I have argued that he did so through a rediscovery of the suppositional necessity explained by Albertus and Aquinas in the late thirteenth century.[32] This doctrine was preserved, and developed, in Jesuit commentaries on Aristotle's *Posterior Analytics*. Especially the teaching on *suppositio*, and how a demonstration made *ex suppositione* could yield a certain truth, seems to have appealed to the young Galileo. Other mathematicians of his day, influenced by Piccolomini, were prepared to reject Archimedes' proof of the law of the balance because it was based on a *suppositio* that was not rigorously true, namely, that perpendiculars drawn from the ends of the balance would be parallel, whereas they would actually converge at the earth's center. Commandino, Benedetti, and Guidobaldo del Monte all subscribed to that view. Galileo departed from them, arguing that the *suppositio* that the lines are parallel need not affect the certitude of the demonstration. Most of his research on motion, in fact, was concerned with experimentally validating the *suppositiones* on which the principles

of uniform motion and of uniformly accelerated motion could be based, so that he could have demonstrations in the science of dynamics paralleling those already worked out by Archimedes in his science of statics.[33]

In the second volume of my *Causality and Scientific Explanation* I have attempted to show how Galileo's ideal, perfected by Descartes, Kepler, and Newton, led to classical mechanics, and its associated planetary astronomy, becoming the science par excellence that would serve as the paradigm for true and certain reasoning down to the end of the nineteenth century.[34] If I am correct, the seventeenth century—*pace* Barbara Shapiro[35]—was the period during which the certitude of science came to be vigorously asserted. A similar claim, in my view, cannot be made for the science that was practiced in the Late Middle Ages and the Renaissance.[36]

NOTES

1. The book's subtitle reads: *A Study of the Relationships Between Natural Science, Religion, History, Law, and Literature,* (Princeton: Princeton University Press, 1983).

2. Steven Marrone, *William of Auvergne and Robert Grosseteste* (Princeton: Princeton University Press, 1983); David Lindberg, *Roger Bacon's Philosophy of Nature* (Oxford: Clarendon Press, 1982); James McEvoy, *The Philosophy of Robert Grosseteste* (Oxford: Clarendon Press, 1982); J. A. Weisheipl (ed.), *Albertus Magnus and the Sciences* (Toronto: Pontifical Institute of Medieval Studies, 1980); and Leo Elders (ed.), *La Philosophie de la nature de Saint Thomas d'Aquin* (Rome: Editrice Vaticana, 1982).

3. R. C. Dales, "The De-Animation of the Heavens in the Middle Ages," *Journal of the History of Ideas*, vol. 41 (1980), pp. 531-50; Edward Grant, "Celestial Matter: A Medieval and Galilean Cosmological Problem," *Journal of Medieval and Renaissance Studies*, vol. 13 (1983), pp. 157-86.

4. W. A. Wallace, "The Scientific Methodology of St. Albert the Great," in *Albertus Magnus Doctor Universalis 1280-1980*, ed. by G. Meyer and A. Zimmerman (Mainz: Matthias Grünewald Verlag, 1980), pp. 385-407. *

5. W. A. Wallace, "Albertus Magnus on Suppositional Necessity in the Natural Sciences," in *Albertus Magnus and the Sciences, op. cit.*, pp. 102-28. *

6. W. A. Wallace, "Aquinas on the Temporal Relation Between Cause and Effect," *Review of Metaphysics*, vol. 27 (1974), pp. 569-84. *

7. John Case, in fact, in his *In universam dialecticam Aristotelis* (London: 1584), p. 178, reproves Averroës himself for not allowing the possibility that the human mind can achieve demonstrative knowledge of any subject matter. Oddly enough, Shapiro does not even mention Case in her study, although he surely was a proponent of certitude for science in the century preceding that of her research. For fuller details, see C. B. Schmitt, *John Case and Aristotelianism in Renaissance England* (Kingston-Montreal: McGill-Queen's University Press, 1983).

8. Edward Grant (ed.), *A Source Book in Medieval Science* (Cambridge, Mass.: Harvard

University Press, 1974), pp. 45-50.

9. W. A. Wallace, *Prelude to Galileo* (Dordrecht-Boston: D. Reidel Publishing company, 1981, Boston Studies in the Philosophy of Science, vol. 62), pp. 341-48.

10. C. A. Wilson, *William Heytesbury: Medieval Logic and the Rise of Mathematical Physics* (Madison: University of Wisconsin Press, 1960), p. 25.

11. M. A. Hoskin and A. G. Molland, "Swineshead on Falling Bodies: An Example of Fourteenth-Century Physics," *The British Journal for the History of Science*, vol. 3 (1966), pp. 150-82, esp. p. 154.

12. Wallace, *Prelude to Galileo, op. cit.*, pp. 54-55, 344-46.

13. Herman Shapiro, *Motion, Time and Place According to William Ockham* (St. Bonaventure: Franciscan Institute Publications, 1957), p. 53.

14. Iohannes Buridanus, *In metaphysicen Aristotelis quaestiones* (Paris: 1518, reprinted Frankfurt a. M.: 1964), fol. 9r.

15. E. A. Moody, *Studies in Medieval Philosophy, Science, and Logic* (Berkeley: University of California Press, 1975), p. 156; Wallace, *Prelude to Galileo, op. cit.*, pp. 230-31, 341-48.

16. Albertus de Saxonia, *Acutissme questiones super libros de physica auscultatione* (Venice: 1516), fols. 36vb-38ra; Wallace, *Prelude to Galileo, op. cit.*, p. 68.

17. Hubert Elie, "Quelques maitres de l'université de Paris vers l'an 1500," *Archives d'histoire doctrinale et littéraire du moyen âge*, vol. 18 (1950-51), pp. 193-243.

18. E.g., Ioannes de Celaya, *Expositio in octo libros phisicorum Aristotelis, cum questionibus...secundum triplicem viam beati Thome, realium, et nominalium* (Paris 1517); Wallace, *Prelude to Galileo, op. cit.*, p. 71.

19. Pierre Duhem, *To Save the Phenomena*, tr. by E. Doland and C. Maschler (Chicago: University of Chicago Press, 1969).

20. W. A. Wallace, "The Problem of Causality in Galileo's Science," *Review of Metaphysics*, vol. 36 (1983), pp. 607-32, esp. 624-25; idem, *Prelude to Galileo, op. cit.*, p. 233.

21. G. C. Giacobbe, "Il *Commentarium de certitudine mathematicarum disciplinarum* di Alessandro Piccolomini," *Physis*, vol. 14 (1972), pp. 162-93.

22. Benjamin Nelson, "The Quest for Certitude and the Books of Scripture, Nature, and Conscience," *The Nature of Scientific Discovery*, ed. by Owen Gingerich (Washington: Smithsonian Institution Press, 1975), pp. 355-72, followed by a discussion, pp. 372-91.

23. W. A. Wallace, *Galileo and His Sources: The Heritage of the Collegio Romano in Galileo's Science* (Princeton: Princeton University Press, 1984), pp. 03-96.

24. W. A. Wallace, "Galileo's Early Arguments for Geocentrism and His Later Rejection of Them," in *Novità Celesti e Crisi del Sapere*, ed. by Paolo Galluzzi (Florence: Istituto e Museo di Storia della Scienza, 1983), pp. 31-40.

25. W. A. Wallace, *Galileo's Early Notebooks: The Physical Questions* (Notre Dame: University of Notre Dame Press, 1977), p. 269.

26. *Ibid.*, p. 268.

27. *Ibid.*, pp. 268-69.

28. Wallace, *Prelude to Galileo, op. cit.*, pp. 325-30.

29. Wallace, *Galileo's Early Notebooks, op. cit.*, p. 96.

30. *Ibid.*, p. 97.

31. J. D. Moss, "Galileo's *Letter to Christina*: Some Rhetorical Considerations," *Renaissance*

Quarterly, vol. 36 (1983), pp. 547-76.

32. W. A. Wallace, "Aristotle and Galileo: The Uses of *Hupothesis (Suppositio)* in Scientific Reasoning," in *Studies in Aristotle*, ed. by Dominic O'Meara (Washington: The Catholic University of America Press, 1981), pp. 47-77. *

33. Wallace, *Galileo and His Sources*, pp. 230-61, 284-91, 322-38.

24. W. A. Wallace, *Causality and Scientific Explanation*, vol. II (Ann Arbor: University of Michigan Press, 1972-74), pp. 03-128.

35. This does not mean that I deny all validity to her thesis; for a fuller appraisal, see my review of her work in *Review of Metaphysics*, vol. 39 (1985-86), pp. 374-77.

36. This paper was presented at the Annual Meeting of the History of Science Society, held in Norwalk, Connecticut, October 28, 1983.

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VIII

Galileo's Sources: Manuscripts or Printed Works?

It has been known since the end of the nineteenth century that two of Galileo's early manuscripts show signs of derivation from other sources, but only in the last decade has any progress been made in identifying what these sources might be. Antonio Favaro, the editor of the National Edition of Galileo's works that began to appear in 1890, speculated then that the manuscripts were nothing more than student notes, and so labeled them *Juvenilia* and assigned them to the periods of Galileo's studies at the Monastery of Vallombrosa and at the University of Pisa.[1] Thus he dated the first of the manuscripts, MS 27 in the Galilean Collection at Florence containing logical questions based on Aristotle's *Posterior Analytics,* in the late 1570s, and the second, MS 46, containing physical questions relating to Aristotle's *De caelo* and *De generatione* and some memoranda on motion, in 1584, at which periods Galileo would have been a youth about fifteen and twenty years old, respectively.[2] His sources at those times, by Favaro's account, would have been a Vallombrosan monk for the logical questions and a Pisan professor, Francesco Buonamici, for the physical questions—although Favaro provided no direct evidence in support of these identifications.[3]

Recent work by Alistair Crombie, Adriano Carugo, Christopher Lewis, William F. Edwards, and myself has called into question such an early dating and the sources it would seem to entail.[4] The sophistication and general erudition of the notes indeed argue against their being composed, or even being excerpted from other sources, by a mere student, however precocious he might be. We have also been able to identify specific texts on which both manuscripts could have been based. In light of these discoveries a wholesale reassessment of the manuscripts and their importance for Galileo's intellectual de-

velopment is now in process. This has given rise to a small problem as to whether Galileo used printed sources when preparing these notes, or whether he based his compositions on other manuscripts available to him—a question that, as we shall see, bears directly on their dating.[5] It is this problem, and a possible solution, that I wish to present in what follows.

From internal evidence it seems clear that MS 27, containing the logical questions, was composed before MS 46 with its physical questions and its memoranda on motion.[6] MS 46, however, had been transcribed by Favaro in the National Edition, whereas MS 27 had not been. Because of this circumstance, the Latin text of the physical questions was the first to be studied, and after considerable investigation it disclosed a number of textual parallels with four textbooks published by Jesuit professors of the Collegio Romano. These are described in the recent literature and may be enumerated as follows: (1) a *Physics* commentary and questionary by Francisco Toledo, printed at Venice in 1573; (2) a *De generatione* commentary and questionary by the same author, printed at Venice in 1575; (3) a work on natural philosophy by Benito Pereyra, printed at Rome in 1576; and (4) a commentary on the *Sphere* of Sacrobosco by Christopher Clavius, printed at Rome in 1581.[7] All of these dates, it should be noted, are prior to 1584, and thus the discovery of these parallels between Galileo's manuscripts and the four printed works does not invalidate Favaro's conjecture that MS 46 was composed while Galileo was yet a student at the University of Pisa in 1584. The parallels, however, account for at most 15 percent of Galileo's entire composition, and they leave open the question of other sources on which the remainder of MS 46 might have been based.[8]

While these investigations were in process, two transcriptions of the Latin text of MS 27 were made independently, by Edwards and Carugo, and these suggest further connections with the Collegio Romano.[9] Galileo's logical questions show clear similarities with Toledo's *Logica,* a textbook published in 1576[10]—only a year or two before Favaro's conjectured date for the writing of MS 27. More surprisingly, however, they show a large number of textual parallels with an *Additamenta* to Toledo's *Logica* that was put into print by a certain Ludovico Carbone, who published this at Venice, but not until 1597.[11] Now if Galileo used Carbone's *Additamenta* to compose his logical questions, he would have been at least thirty-three years old, at the height of his teaching career at the University of Padua. Moreover, since the logical questions quite clearly antedate the physical questions, this would mean both of the manuscripts in ques-

tion—i.e., MSS 27 and 46—could not have been written by Galileo until after 1597, which would be close to twenty years later than Favaro first speculated for their composition. A yet further complication is introduced by the fact that the memoranda on motion contained in MS 46 form the basis for several treatises *De motu* found in another of Galileo's manuscripts, MS 71, which scholars agree was composed between 1590 and 1592, while Galileo was still teaching mathematics at the University of Pisa.[12] Thus to claim that MSS 27 and 46 were based exclusively on printed sources presents a number of difficulties. MSS 46 and 71 could have been composed prior to 1592, at which time Galileo moved from Pisa to Padua, but MS 27, which gives clear indication of having been written *before* the other two, could not have been written until 1597, and this would be a full five years after the move.

Such difficulties notwithstanding, in a recent communication at an International Galileo Congress Crombie and Carugo have taken the position that the main source of MS 27 actually was Carbone's *Additamenta,* and that the consequent late dating of that manuscript necessitates a wholesale revision in the accepted chronology of Galileo's writings and the place of his scholastic compositions within them.[13] Rather than see MSS 27 and 46 as student lecture notes, the authors contend—from their later dating and the mature style of arguing in the manuscripts—that they provide "a systematic and learned account of a series of philosophical questions and disputations compiled by an experienced scholar whose interests are focused on the theory of science and demonstrative knowledge and on cosmology and natural philosophy."[14] Such a stance requires them to find a new date for the composition of the treatises on motion contained in MS 71. These they now would assign various dates: the earliest among them they admit could come from Galileo's Pisan days, but the later versions they hold were not completed until after his *Meccaniche,* which they would now date sometime between 1615 and 1623. The treatises on cosmology and natural philosophy contained in MS 46 were then compiled, on their reckoning, either "while writing or just after publishing the *Saggiatore,* the first published work in which Galileo indulges in scholastic disputations."[15] This new estimate of notes Favaro had labeled *Juvenalia* puts them in reasonable proximity to the writing of the *Dialogo* on the two chief world systems, many of whose discussions "can be traced to scholastic commentaries circulated or produced by Jesuits between the end of the 16th and the beginning of the 17th century."[16] So Crombie and Carugo see the famous *Dialogo* of 1632 as actually based on two

models: those "of Plato's dialogues and of the scholastic disputation revived by the Jesuits and practiced by Galileo himself in his scholastic dissertations."[17]

This, to be sure, is revisionism of a drastic sort, and one wonders if such an extreme thesis can eventually be supported. So let me return to a consideration of MS 46, to see if an alternate course might be available that would not call into question so much of Galileo's hitherto accepted scientific biography—such as is set forth, for example, in Stillman Drake's *Galileo at Work*.[18] As I explain in my *Galileo's Early Notebooks* (p. vii), already in 1972 I had begun to wonder why only 15 percent of this manuscript shows parallels with the printed texts of Toledo, Pereyra, and Clavius already mentioned. My study of corrections, deletions, and other signs of copying in the manscript then led me to wonder whether some of them were occasioned by Galileo's working from handwritten materials he had difficulty deciphering. Operating on a hunch, I ordered microfilms of Pereyra's lectures on natural philosophy that I had noted were preserved in manuscript *reportationes* in the Oesterreichische Nationalbibliothek.[19] This proved to be a happy inspiration, for I found that Pereyra's lectures contained considerably more material than he printed in his textbook, and that much of the new material had counterparts in Galileo's manuscript. That put me on the track of other *reportationes* of courses at the Collegio, by professors whose notes never did find their way into print, but whose lectures, on inspection, I further found could account for an additional 75 percent of Galileo's composition.[20] The new parallels, some of which are reproduced in my *Prelude to Galileo*, (pp. 213–16, 256–81) and others in my *Galileo and His Sources* (pp. 70–89), also served another purpose: they provided strong evidence for dating Galileo's writing of MS 46 around 1590.[21] The closest textual coincidences, in fact, are
* with the notes of a Jesuit professor, Paolo Valla, who taught the course in natural philosophy at the Collegio in 1588–89 and whose notes were not available until late in 1589. This later dating, some six years beyond that proposed by Favaro, fits in well with the previously noted connection between MS 46 and MS 71, since it confirms that both were written while Galileo was teaching at the University of Pisa. Thus the first is not a student composition, as Favaro thought, and although it was written at Pisa, not in the way he had conjectured.

That still leaves the problem of dating MS 27, containing Galileo's logical questions. Fortunately there is a *rotula* of professors who taught at Rome in various years, and this shows that Paolo Valla had the logic course in 1587–88—that is, the year immediately preceding

his teaching of the physical questions.[22] Not only this, but the handwritten list indicates that a professor named "Ioannes Lacerino" taught logic at the Collegio two years before Valla, or in 1585–86.[23] Subsequent checking turned up a Latin manuscript in the Vatican Library full of logic notes ascribed to a certain "Ioannis Laurinis S.I." with the notation that the latter had taught logic in Rome in 1584.[24] In the family name recorded on the manuscript, moreover, the letters "au" had been crossed out and the letter "o" written above them, thus correcting the author's name to Lorinus (or, in the Italian, Lorini). Now Lorini, it turns out, developed into a Scripture scholar and later produced a fair number of publications, among which is a textbook on logic printed in 1620.[25] It took me some time to locate this published *Logica* and to compare it with the manuscript version in the Vatican Library, but when I finally succeeded in doing so the result was surprising.[26] The published course was almost exactly the same as that existing in manuscript some thirty-six years previously!

Apparently this was not an isolated instance, for a search through the card files of Italian libraries revealed that Paolo Valla had also published his entire course in logic based on the lectures given in 1588–89. This appeared in two massive folio volumes entitled *Logica,* printed at Lyons, but not until 1622.[27] Such a late date was somewhat discouraging, and so was the material contained in the tomes. Somewhat like Lorini's text, yet in far more exhaustive detail, they covered the same ground as did Galileo's logical questions, but not in precisely the same words. With repeated checking it soon became clear that I would not find parallels in Valla's printed text superior to those already identified in Carbone's *Additamenta* of 1597—a work published twenty-five years previously, and already too late for the purposes I had in mind.

Somewhat discouraged, I decided to translate the prefaces to the two volumes of Valla's *Logica* to see if they might cast light on the perplexing situation. Much to my surprise they did.

In his remarks *Ad lectorem* at the beginning of volume 1, after first outlining the contents of the volume, Valla explains that he will start by prefacing a brief introduction to the whole of logic. Of this he writes:

> We preface, I say, an *Introductio* that was explained by us thirty-four years ago [i.e., in 1588] in the Collegio Romano and given to our hearers shortly thereafter. This work, with very little of the fruits of our labors changed in it, was published at Venice by some good author, who added some preliminary matter and made some inversions (or rather perversions) of its order that, in my judgment, achieve no better results. We wish to warn

> you the reader of this, so that, should you come across this book, you will recall that he took it from us. And since he stole this and similar matter from us and from the writings of our Fathers [i.e., other Jesuits] perhaps he should have added the author's name to these books, had he known it or thought it due us.[28]

Valla then announces that his second volume will contain his expositions of the *Prior Analytics* and *Posterior Analytics,* and to this he will add a *Disputatio de scientia,* of which he further remarks:

> The same thing happened to this *Disputatio* as I explained happened to the *Introductio.* But this we have now so enlarged and perfected that it would hardly be recognized by anyone except the author as the fetus of the same.[29]

The intimation of plagiarism, of course, was quite unexpected, all the more as it related to Valla's *reportationes* of 1588, in which I was very interested. No less exciting was the preface to volume 2 of the *Logica.* By the time he came to its composition Valla had decided that he would append four complete tractates to his commentaries on the *Analytics* rather than *De scientia* alone. These he now enumerates as *De praecognitionibus, De demonstratione, De definitione,* and *De scientia.* Concerning the order of these tractates, he alerts the reader to the following:

> About twenty years ago [i.e., around 1600], a certain individual—possessing a doctorate, having published a number of small books, and being otherwise well known—had a book printed at Venice in which he took over and brought out under his own name a good part of what we had composed in our *De scientia* and had taught at one time, thirty-four years before this date [i.e., in 1588], in the Roman *gymnasio.* And having done this, this good man thought so much of other matters we had covered in our lectures that he took from them, and claimed under his own name, a large part of *De syllogismo, De reductione, De praecognitionibus,* and *De instrumentis sciendi,* and proposed these as kinds of *Additamenta* to the logic of Toledo, especially to the books of the *Prior Analytics.* He also saw fit to publish, again under his own name, our *Introductio* to the whole of logic, having changed only the ordering (disordering it, in my judgment), along with the introductions and conclusions. I wish you to know this, my reader, so that, should you see anything in either, you will recognize the author. I say, "should you see anything in either," for we have so expanded our entire composition that, if you except only the opinions (which once explained we have not changed), hardly anything similar can you see in either. So in those works you have what he took from me, in this what I have prepared more fully and at length.[30]

Here, then, was the solution to my puzzle. The "good man" to whom Valla made reference was no doubt Ludovico Carbone, and it was a simple matter to check on the latter's literary output in 1597—agreeing roughly with Valla's estimate that the works were published around 1600—to find there all the materials Valla had mentioned in his prefaces.[31] Carbone's *Additamenta*, with which Galileo's logical questions show such remarkable agreement, were nothing more than Valla's lecture notes of 1588. And if this is true, so were Galileo's logical questions contained in MS 27, which parallel Carbone's 1597 version so closely that there can be little doubt of their derivation from a common source.

Carbone, then, was a plagiarist, as Valla clearly attests. Was Galileo a plagiarist also? I would say he was not, though the press has attributed the opposite opinion to me.[32] Even though Galileo cribbed from the same source, the notes he took were for his personal use, and he never made Carbone's mistake of putting them into print.

The dating problem posed earlier in this essay yields quickly to solution on the basis of this same evidence. MSS 27, 46, and 71 were composed in that order, beginning probably in 1589 (since Valla's logic course was not completed until August of 1588, and he did not give out his notes until "shortly thereafter"), and finishing equally probably in 1592, before Galileo's move to Padua, which places the writing of all three manuscripts during Galileo's teaching career at Pisa. Not only was MS 27 based on a handwritten *reportatio*, but so was MS 46, although the *reportationes* on which it was based were partially excerpted from the printed texts of Toledo, Pereyra, and Clavius—which explains the 15 percent of textual parallels with those works in Galileo's manuscript.

This, then, is my answer to the question posed in my title: Galileo's sources, manuscripts or printed works? His primary sources were handwritten, although portions of them probably incorporated materials that had already found their way into print. Taking the manuscript evidence into account, one need not subscribe to the radical revisions in chronology proposed by Crombie and Carugo, while one can still account for many of the scholastic influences manifest in Galileo's later writings to which they call attention. In this way the more-or-less established dating of the remainder of Galileo's works is preserved, and yet the heritage of the Collegio Romano is seen as one of the more important factors shaping the overall development of Galileo's science.[32]

In conclusion, I would like to make two observations that relate to the subject of print and culture in the Renaissance. The first has to do with the long time delay between the preparation of notes or *reporta-*

tiones of Jesuit lectures at the Collegio Romano and their eventual appearance in print. Toledo's course on logic was given at the Collegio in 1560, and his textbook did not appear until 1576—sixteen years later. Giovanni Lorini finished his logic course at the Collegio in 1584, and his textbook was not published until 1620, at Cologne, thirty-six years later. Valla's logic course, as we have seen, was completed in 1588, plagiarized in 1597 (nine years later), and then published by its author in revised form in 1622 (thirty-four years after its composition). And Pereyra's course in natural philosophy was in manuscript form in 1566, but portions of it did not come into print until 1576, after a delay of ten years. I wonder if these publication delays in the late sixteenth century are representative, and if so, what they might be able to tell us about the currency of ideas then appearing in print, when they had been communicated to students as much as three and a half decades prior to their publication.

My second observation is simply this. Paul Oskar Kristeller has repeatedly emphasized how essential it is to study manuscript traditions long after the invention of printing if one is to gain a complete understanding of sixteenth-century thought. Now I should like to emphasize conversely how important it is to study printed works, and especially their ornate and sometimes frustrating Neo-Latin prefaces, if one is to have a full understanding of sixteenth-century manuscripts. The two types of reproduction, pen and print, were so interdependent in that century that one risks, at great peril, the neglect of either as a likely source of information.[33]

Notes

1. Favaro's judgment about the manuscripts is reflected in his introductions to the works appearing in the National Edition, viz., Antonio Favaro, ed., *Le Opere di Galileo Galilei,* 20 vols. in 21 (Florence: G. Barbèra Editore, 1890–1909), reprinted 1968. As will be explained below, Favaro transcribed the codex dealing with cosmology and theories of the elements and published this in the first volume with the title *Juvenilia,* along with the early treatises *De motu.* He somewhat arbitrarily excluded from the edition another codex treating of logical matters, giving only a brief description of it in the ninth volume along with other "scholastic exercises" of the youthful Galileo. Effectively, therefore, he regarded the manuscripts as student notes, some of which he thought worthy of being included in the Galilean corpus, others not.

2. The bulk of Galileo's extant handwritten materials is preserved in the Galileo Collection of the Biblioteca Nazionale Centrale in Florence, but the present numbering of the manuscripts does not correspond to that given in the National Edition. A useful guide to the sources, which provides both sets of numbers, is Eugenia Levi, *Indice delle Fonti dell'Edizione Nazionale delle Opere di Galileo Galilei* (Florence: G. Barbèra Editore, 1968). For Favaro's dating of MS 27, consult the National Edition, 9:279–82; for that of MS 46, see 1:9–13.

3. Favaro's speculation about the Vallombrosan sources of the notes in MS 27 is traceable to an early biography of Galileo written by one of his students, Vincenzo Viviano, which is reprinted in the National Edition; see 9:279, and 19:602. His conjecture about Buonamici is based mainly on the list of professors teaching at the University of Pisa while Galileo was a student there; see 1:12. Some additional details are given in A. Favaro, "Galileo Galilei e i Doctores Parisienses," *Rendiconti della R. Accademi dei Lincei* 27 (1918): 3–14; see also my critique of Favaro's reasoning in my essay with the same title, reprinted in *Prelude to Galileo,* (n. 4 below), 192–252.

4. This work has been in progress for over fifteen years, though as yet its results are not widely known. The pioneering efforts of Crombie and Carugo are described in A. C. Crombie, "Sources of Galileo's Early Natural Philosophy," in *Reason, Experiment, and Mysticism in the Scientific Revolution,* ed. M. L. Righini Bonelli and W. R. Shea (New York: Science History Publications, 1975), 157–75 and 303–5. Related research, reported in fuller detail, is described by Christopher Lewis, *The Merton Tradition and Kinematics in Late Sixteenth- and Early Seventeenth-Century Italy* (Padua: Editrice Antenore, 1980). My early investigations are summarized in *Prelude to Galileo: Essays on Medieval and Sixteenth-Century Sources of Galileo's Thoughts* (Dordrecht-Boston: D. Reidel Publishing Company, 1981), and in *Galileo's Early Notebooks: The Physical Questions. A Translation from the Latin, with Historical and Paleographical Commentary* (Notre Dame, Ind.: University of Notre Dame Press, 1977). My most recent study, *Galileo and His Sources: The Heritage of the Collegio Romano in Galileo's Science* (Princeton: Princeton University Press, 1984), is the basis for much of this essay. For Edwards's contribution, see n. 9 below.

5. The problem was first formulated during informal discussions I had with Adriano Carugo at Milan in 1975 and at Padua in 1981; it emerged more clearly at the International Congress on Galileo held in Italy during March 1983 (see n. 13 below for further details).

6. This evidence is discussed fully in *Galileo and His Sources,* 3–96; some details are adumbrated in my commentary on the physical questions, *Galileo's Early Notebooks,* 255, 273, and 278–79.

7. For the Latin titles, see *Galileo's Early Notebooks,* 13.

8. See *Prelude to Galileo,* 194–217, esp. 200.

9. At this writing, neither of these transcriptions has appeared, although Carugo has indicated that a substantial portion of his text will be printed, together with an English translation, in a book he is coauthoring with Crombie. Edwards is currently readying his entire text for publication; parts of this have been used in preparing *Galileo and His Sources,* and some excerpts appear in my essay "The Problem of Causality in Galileo's Science," *The Review of Metaphysics* 36 (1983): 607–32.

10. Franciscus Toletus, *Commentaria, una cum quaestionibus, in universam Aristotelis logicam . . .* (Venice, 1576). A 1596 edition of this work, published at Cologne, was among the works in Galileo's personal library; see *Galileo and His Sources,* 123, n. 74.

11. The full title is *Additamenta ad commentaria D. Francisci Toleti in Logicam Aristotelis. Praeludia in libros Priores Analyticos; Tractatio de Syllogismo; de Instrumentis sciendi; et de Praecognitionibus, atque Praecognitis. Auctore Ludovico Carbone a Costacciaro . . .* (Venice, 1597). The more striking parallels are exhibited in *Galileo and His Sources,* 33–51.

12. For a discussion of the memoranda and their relation to the treatises on motion in MS 71, see Raymond Fredette, "Galileo's *De motu antiquiora,*" *Physis* 14 (1972): 321–48. Additional information is provided in Fredette's unpublished paper, "Bringing to Light the Order of Composition of Galileo Galilei's *De motu antiquiora,*" delivered at a workshop on Galileo in Blacksburg, Virginia, in 1975.

13. See A. C. Crombie and A. Carugo, "The Jesuits and Galileo's Idea of Science

and of Nature," presented at a Convegno Internazionale di Studi Galileiani entitled *Novità Celesti e Crisi del Sapere* and held at Pisa, Venice, Padua, and Florence on 18–26 March 1983. An abstract of the paper was published in the *Sommari degli Interventi* (Florence: Banca Toscana, 1983), 7–9.

14. Crombie and Carugo, *Sommari degli Interventi* (abstract), 8.
15. Ibid., 9.
16. Ibid.
17. Ibid.
18. Chicago: University of Chicago Press, 1978.
19. Fortunately the existence of these manuscripts was noted in Carlos Sommervogel et al., *Bibliothèque de la Compagnie de Jésus*, 11 vols. (Brussels-Paris: Alphonse Picard, 1890–1932), 6:499–507. They are described in *Galileo's Early Notebooks*, 307, n. 12.
20. *Galileo's Early Notebooks*, vii, 13.
21. *Prelude to Galileo*, 217–28, and *Galileo and His Sources*, 89–95.
22. Names of professors teaching the various courses are given in an appendix to R. G. Villoslada, *Storia del Collegio Romano dal suo inizio (1551) alla soppressione della Compagnia di Gesù (1773)* (Rome: Gregorian University Press, 1954), 321–36; the appendix was prepared by Ignazio Iparraguirre. A fuller listing is provided in *Galileo and His Sources, 7*.
23. Villoslada, *Storia del Collegio Romano*, 331. The reading "Lacerino" given in Villoslada seems to be a misreading of what was intended to be "Laurino," as explained in what follows.
24. Cod. Urb. Lat. 1471. The first folio contains the name of the author, and the last, fol. 560r, notes the year in which the course was completed, MDLXXXIV.
25. For details, see Sommervogel et al., *Bibliothèque de la Compagnie de Jésus*, 6:cols. 1–6, and C. H. Lohr, "Renaissance Latin Aristotle Commentaries: Authors L–M," *Renaissance Quarterly* 31 (1978): 544.
26. Ioannes Lorinus, *In universam Aristotelis logicam* (Cologne, 1620).
27. The fuller title reads *Logica Pauli Vallii Societatis Iesu duobus tomis distincta: Quorum primus artem veterem, secundus novam comprehendit.*
28. *Logica*, 1:fol. 4r. The Latin texts for this and later citations are given in *Galileo and His Sources;* 18–19, nn. 32–34; the English translations here and hereafter are mine.
29. *Logica*, 1:fol. 4r.
30. Ibid., 2:fol. 1.
31. All of these works are in the Vatican Library; their titles are listed in *Galileo and His Sources*, 13, n. 23.
32. A feature story by Philip J. Hilts in *The Washington Post* of 23 February 1981, transmitted by international wire services and widely disseminated both here and abroad (usually with headlines that went far beyond the content of the article), incorrectly attributed to me the belief that Galileo had plagiarized ideas from Jesuit authors.

* 33. The broad outline of that development is sketched in my "Aristotelian Influences on Galileo's Thought," in *Aristotelismo Veneto e Scienza Moderna*, ed. L. Olivieri, 2 vols. (Padua: Editrice Antenore, 1983), 1:349–78. Fuller details will be found in *Galileo and His Sources*, 219–349.

34. The research on which this essay is based was supported in part by the National Science Foundation, Research Grant 79-24825.

IX

GALILEO'S CONCEPT OF SCIENCE: RECENT MANUSCRIPT EVIDENCE

Introduction

Since the end of the nineteenth century it has been suspected that two of Galileo's early Latin manuscripts, one containing questions on logic (MS 27) and the other questions on the heavens and the elements, plus some memoranda on motion (MS 46), were derived and possibly copied from other sources. [1] The editor of the National Edition of Galileo's works that began to appear in 1890, Antonio Favaro, speculated then that both were student notebooks — the first written by Galileo while he was at the Monastery of Vallombrosa in the late 1570s and the second while he was studying at the University of Pisa in 1584. [2] Favaro transcribed the second manuscript and published its transcription under the title *Juvenilia* in the first volume of the National Edition. [3] The first manuscript he regarded as so insignificant that he excluded it from the edition, merely transcribing a few excerpts as "samples of some scholastic exercises of Galileo" and putting these in the ninth volume with other data pertinent to Galileo's youth. [4] A third manuscript, however, which contains drafts of Galileo's early writings on motion (MS 71), he did transcribe and published in its entirety, while rearranging the writings to conform to his idea of how they were composed. This last manuscript Favaro fortunately assigned to the period of Galileo's teaching at Pisa, *ca.* 1590. [5] Of all Galileo's Latin compositions it has caught the attention of scholars since it is obviously related to his later writings *De motu,* on which his fame as "Father of Modern Science" rests.

Because of Favaro's dating and handling of these three manuscripts in the National Edition, the connections between them have been overlooked. Recent scholarship, partially inspired by renewed interest in medieval science, has redirected attention to them and has yielded some interesting results. The third manuscript (MS 71) was the first to be studied, by Raymond Fredette, in an attempt to understand the ordering of its materials. [6] Not only was Favaro's arrangement found to be questionable, but the entire contents were discovered to be a progressive development of the memoranda on

motion following the questions on the elements in the second manuscript (MS 46). Then this second manuscript was subjected to close scrutiny and its sources gradually uncovered. Most of this detective work is described in my *Galileo's Early Notebooks: The Physical Questions* and *Prelude to Galileo.*[7] It reveals the dependence of MS 46 on the lecture notes of young Jesuit professors who were teaching in Rome, at the Collegio Romano, at about the same time as Galileo was beginning his teaching career at the University of Pisa. More importantly, it establishes that the manuscript was written, as an earlier curator of the Galileiana collection had indicated, "around 1590."[8] This, of course, makes MS 46 contemporaneous with the *De motu antiquiora* of MS 71 and serves to explain the curious relationship of its memoranda on motion to the contents of the longer work.

Finally, the first manuscript (MS 27) has been recovered from
* the oblivion to which Favaro consigned it.[9] The study of this is still in progress, but preliminary indications are that it contains the greatest surpise of all. Instead of being based on the teachings of a Vallombrosan monk around 1578, as Favaro conjectured, the manuscript contains Galileo's adaptations of portions of Jesuit commentaries on Aristotle's *Posterior Analytics,* which my analysis shows could not have been completed before August of 1588.[10] Apart from the important treatment of scientific methodology it contains, this manuscript provides evidence that Galileo was seriously studying Jesuit course materials on logic and natural philosophy while preparing for, or actually occupying, his first teaching post at Pisa between 1589 and 1591. All three manuscripts (MSS 27, 46, and 71) therefore date from approximately the same period — actually one of great productivity for Galileo, during which he laid the foundations on which his later work would be based.[11]

The Collegio Romano

In view of this use by Galileo of Jesuit teaching notes, a brief sketch of their origin and contents would seem indicated at this point. As is well known, the Collegio Romano (or Roman College) was founded by St. Ignatius Loyola himself in 1551.[12] It quickly grew to a position of prominence and prestige, so that by the end of the 1580 it had become the foremost university run by the Jesuits in all of Europe. The early professors of philosophy at the Collegio were mainly Spaniards, the most influential being Francisco Toleto (Franciscus Toletus), who had studied under Domingo de Soto at Salamanca before becoming a Jesuit, and Benedetto Pereira (Benedictus Pererius), a Valencian who was later to make his mark as a Scripture scholar. Both wrote manuals of philosophy that were first published in the 1570s and reprinted often thereafter, although they

last taught such courses themselves in the 1560s.[13] Toleto's texts are important because they were supplemented, and improved upon, in the lecture notes of later Jesuits, one set of which, as we shall see, was published as *Additamenta* (or additions) to Toleto's logic as late as 1597. Pereira's writings are similar, and his textbook on natural philosophy, *De communibus omnium rerum naturalium principiis et affectionibus,* published at Rome in 1576, exerted considerable influence. More eclectic and less Thomistic than Toleto, Pereira subscribed to a number of Averroist theses, among which was a strongly expressed opposition to the use of mathematics in the study of nature. His Averroism plus differences of opinion with Christoph Clavius, the mathematics professor at the Collegio who urged the use of mathematics in physics, may explain his later "promotion" to the Scripture faculty of that institution — *promovetur ut amoveatur*, as the Romans would say.

Apart from the textbooks produced by Toleto and Pereira, there is little published information about the materials covered in course work at the Collegio. Fortunately, however, a large number of extant manuscripts contain the lecture notes or *reportationes* of lectures of later Jesuits there, and these are a rich source of data on this subject.[14] For purposes of this presentation, the notes of Antonius Menu mark the indispensable starting point for the study of influences on Galileo.[15] As can be seen in Table I, which contains a list of Collegio professors and the courses they taught, Menu lectured on natural philosophy and metaphysics from 1577 to 1579, and then taught logic, natural philosophy, and metaphysics again from 1579 to 1582.[16] Menu's first appearance in the *Physics* course came only one year after Pereira's *De communibus* was published, but at that time, as we know from his lecture notes, Menu broke radically with Pereira's theses.[17] Instead of adopting a conservative Averroist stance, as Pereira and most Italian professors in neighboring universities had done, Menu imported into a general Thomistic framework a progressive Aristotelianism that owed much to the *Doctores Parisienses* and to the fourteenth-century "calculatory" tradition of Oxford and Paris. On this account he was more open to the use of mathematics in physics than was Pereira, and certainly was more acceptable to Clavius on that account.

Many of Menu's ideas in natural philosophy, and particularly his teachings on *impetus*, were taken up by a successor, Paulus Valla *
(Vallius), who taught the tract on the elements, *De elementis,* as part of the metaphysics course in 1585-1586 and again in 1586-1587.[18] Then, in 1587, Valla began a sequence that was to become quite usual at the Collegio, wherein each professor would take his class through the entire three years of the philosophy cycle. Valla taught logic in 1587-1588, then natural philosophy in 1588-1589, and finally metaphysics again (for him the third time) in 1589-1590. As can again be seen in Table I, Mutius Vitelleschi pursued that cycle in the

years 1588 through 1591, and then Ludovico Rugerio (Ludovicus Rugerius) did the same in the years 1589 through 1592. In the cases of Vitelleschi and Rugerio a complete record of their lectures in philosophy survive, and these are in essential continuity with the portions of the courses of Menu and Valla that are still extant.[19] Rarely would one professor repeat his predecessor's positions word-for-word, and signs of disagreement within the faculty are not totally absent, but on the whole there is remarkable consensus among them. This is particularly true of most of the matters that show up in Galileo's early Latin manuscripts. Strong evidence has accumulated, as already noted, to show that the contents of these manuscripts were appropriated from the lecture notes of Valla, and possibly his colleagues, at the very time when Galileo was launching his own teaching career at the University of Pisa.[20]

TABLE I

PROFESSORS AND COURSES IN PHILOSOPHY OFFERED AT THE COLLEGIO ROMANO

YEARS	LOGIC	NATURAL PHILOSOPHY	METAPHYSICS
1577-1578	—	A. Menu	—
1578-1579	—	—	A. Menu
1579-1580	A. Menu	—	—
1580-1581	—	A. Menu	—
1581-1582	—	—	A. Menu
1582-1583	—	—	—
1583-1584	I. Lorinus	—	A. Parentucelli
1584-1585	—	M. De Angelis	A. Parentucelli
1585-1586	I. Lorinus	M. De Angelis	P. Valla
1586-1587	I. Caribdi	M. De Angelis	P. Valla
1587-1588	P. Valla	I. Caribdi	M. De Angelis
1588-1589	M. Vitelleschi	P. Valla	I. Caribdi
1589-1590	L. Rugerius	M. Vitelleschi	P. Valla
1590-1591	A. De Angelis	L. Rugerius	M. Vitelleschi
1591-1592	R. Jones	A. De Angelis	L. Rugerius

Galileo's Dependence on Valla

The dependence of Galileo's MS 27, containing his questions on Aristotle's *Posterior Analytics,* on Valla's logic notes has not been easy to determine, but in what follows I shall sketch the line of research on which it is based.[21] MS 27 begins with a *tractatio* or treatise entitled *De praecognitionibus et praecognitis in particulari*, which translates as "On foreknowledges and foreknowns in particu-

lar", and apparently has been preceded by some folios, now missing, concerned with foreknowledges and foreknowns in general.[22] The title is not a common one, but it is obviously part of a commentary or "questionary" on the *Posterior Analytics,* the first chapter of which is concerned with this very topic. Search through many manuscripts and printed books finally yielded one book whose table of contents lists questions like this, and indeed gives many other titles that correspond to the remaining disputations and questions contained in MS 27. The index, in fact, lists a number of titles pertaining to a *Tractatio de praecognitionibus et praecognitis*, in coverage quite like Galileo's, and then also enumerates questions for a *Tractatio de instrumentis sciendi*, that is, a treatise on instruments of knowing. The latter tract is also somewhat odd, being concerned with such topics as definition, demonstration, resolution and composition, etc., and inquiring which is the more important for generating scientific knowledge. Quite remarkably, selected passages in the treatise on foreknowledge parallel very closely Galileo's exposition in MS 27, suggesting a genetic connection between the two. But the book, it turns out, was printed at Venice in 1597, and its author, Ludovico Carbone, proposes it as certain *Additamenta* to the logic text of Toleto, already mentioned.[23] The date, one will recognize, is quite late — 1597 — a full seven years *later* than that MSS 46 and 71, whereas MS 27, by all other indications, should have preceded the other two in order of composition.

This enigma persisted until, in my search for writings of Jesuit professors (and Carbone was not a Jesuit, although he had studied under them), I came across a two-volume logic text published by Valla at Lyons in 1622.[24] This also listed treatises on these very same subjects, though the wording was not as close to Galileo's as that found in Carbone. One day, almost by accident, I decided to translate the preface to Valla's second volume, and came across the following passage, which reads:

> About twenty years ago [i.e., around 1602], a certain individual — possessing a doctorate, having published a number of small books, and being otherwise well known — had a book printed at Venice in which he took over and brought out under his own name a good part of what we had composed in our *De scientia* and had taught at one time, thirty-four years before this date [i.e., in 1588], in the Roman *gymnasio*. And having done this, this good man thought so much of other matters we had covered in our lectures that he took from them, and claimed under his own name, a large part of *De syllogismo, De reductione, De praecognitionibus*, and *De instrumentis sciendi*, and proposed these as kinds of *Additamenta* to the logic of Toletus, especially to the books of the *Prior Analytics*. He further saw fit to publish, again under his own name, our *Introductio* to the whole of logic, having changed only the ordering (disordering it, in my judgment), along with the introductions and conclusions. I wish you to

> know this, my reader, so that, should you see anything in either, you will know the author. I say, "should you see anything in either", for we have so expanded our entire composition that, if you except only the opinions (which once explained we have not changed), hardly anything similar can you see in either. So in those works you have what he took from me, in this what I have prepared more fully and at length. [25]

This piece of information, to be sure, changed the whole picture. Carbone, through his plagiarism, had unwittingly preserved Valla's logic course at it was offered at the Collegio Romano in 1587-1588, which we know was not completed until August of 1588. Galileo, through the good graces of Clavius — and concerning this, more later — obtained a copy of Valla's lecture notes, and from these wrote out the interesting materials contained in MS 27. This he probably did early in 1589, and thus it is that all three of his Latin manuscripts (MSS 27, 46, and 71) date from the same period, probably written in succession at Pisa, in the years 1589 to 1591.

Texts and Contents

To gain some idea of the extent of the correspondences between Galileo's composition and the text of Valla-Carbone, as I shall henceforth refer to it, I have transcribed some 350 lines of Galileo's manuscript (it contains 1834 lines in all) and placed them in parallel column with the wording of Valla-Carbone. Moreover, I have italicized all words that are either the same or are synonymous in the two compositions. Apart from the ordering of the passages, which I have had to rearrange in some cases, the treatments are so close as to leave little doubt of derivation from a common source. [26]

An illustration will serve to make the case. On one of the folios of MS 27 Galileo has a marginal insert. [27] The insert occurs at Galileo's *Secunda conclusio* for the question he is answering, which reads: *In scientiis realibus praecognoscendum est esse existentiae actuale de subiecto demonstrationis* (i.e., in real sciences the actual *esse existentiae* of the subject of demonstration must be foreknown). Here there is a sign for a marginal addition that reads: *saltem suis locis et temporibus, remotis suis impedimentis* (i.e., at least for its places and times, when impediments have been removed). When this passage had been located in Valla-Carbone, it yielded a tell-tale trace that offers pretty good proof of copying. [28] Table II shows the first three conclusions of this question, arranged in parallel columns, with Valla-Carbone on the left and Galileo on the right. [29] Not only does Valla-Carbone have the inserted expression, *suis saltem locis et temporibus, remotis impedimentis*, but it actually repeats the first part of the expression and explains why it is important for a correct understanding of the conclusion. Galileo, in his haste or in his

attempt to abbreviate, had deleted the expression in his version of the notes. Reading on, and later seeing that the omitted qualification was important, he did what any intelligent scribe would do — he inserted it in the margin of the manuscript.

TABLE II

TEXTUAL PARALLELS BETWEEN VALLA-CARBONE AND GALILEO

VALLA CARBONE	GALILEO
Prima positio, de subiecto praecognoscendum est esse essentiae antequam passio de illo probetur. ...Confirmatur... Secundo, *quia* hoc demonstratur in conclusione, sed *non potest demonstrari nisi* de subiecto *praecognoscamus esse essentiae,* ergo. ...	*Prima conclusio: esse essentiae praecognoscendum est de subiecto,* ita ut, *nisi praecognoscatur, nulla potest haberi demonstratio.* Probatur ex argumentis primae opinionis.
Secunda positio, in scientia reali debet praecognosci esse existentiae actuale, suis saltem locis et temporibus, remotis impedimentis. Dicimus 'suis locis et temporibus' quia multae sunt naturae quae non habent individua semper actu existentia, et ideo de his satis est praecognoscere quod suis temporibus existant. ...	*Secondua conclusio: in scientiis* * *realibus praecognoscendum est esse existentiae actuale* [Margin: *saltem suis locis et temporibus, remotis* suis *impedimentis*] de subiecto demonstrationis in quo vel passio ostendatur de illo vel aliquid aliud praedicatum. ...
Tertia positio, non est opus ante omnem demonstrationem praecognoscere de subiecto esse actuale existentiae. ... Primo... Secundo, *in aliquibus demonstrationibus probatur subiecti existentia, ut* cum probatur existentia causae per effectum, *ut Deum et materiam esse:* igitur non supponitur esse. ...	*Tertia conclusio: in scientiis non semper de subiecto praecognoscendam esse existentiam actualem.* Patet *in illis demonstrationibus in quibus ostenditur existentia inesse subiecto: ut* videre est in illis in quibus aut *materia* prima *existere* per transmutationem aut *primum motorem dari* ex motus aeternitate probatur. ...

Working in this way through the Valla-Carbone text, one can correlate all of Galileo's MS 27 with either it or with Valla's *Logica* of 1622. I have made these correlations, and indicate them schematically in Table III for the various treatises and questions found in MS 27. Reference to that table will show that the two treatises in the manuscript are somewhat unequal in length. The Treatise on Foreknowledge, occupying folios 4 through 13, contains eleven questions, discussing successively the foreknowledges required of principles, subjects, and properties on the part of one who wishes to demonstrate, whereas the Treatise on Demonstration, running from folio 13 through folio 31, contains sixteen questions, dealing successively with the nature, properties, and kinds of demonstration. The most extensive and detailed correlations are those between Galileo's first eleven questions and the *Tractatio de praecognitionibus* of the *Additamenta* published in 1597. Similar materials can be found in the second volume of Valla's *Logica* of 1622, but rarely is the wording there exactly the same as in the manuscript. The Treatise on Demonstration, on the other hand, does not appear in the *Additamenta*, never having been plagiarized by Carbone, though it does have parallels in the *Logica* of 1622. One can notice, in fact, that the ordering of Galileo's questions in MS 27 follows closely that of the *Logica*, which probably reflects the original ordering of Valla's lecture notes. The *Additamenta*, on the other hand, departs from it — precisely the point made by Valla in the prefaces to the 1622 work. Further confirmation of this is seen in the second question of the disputation on the nature and importance of demonstration, which is concerned with demonstration as an instrument of knowing. This particular question was plagiarized by Carbone and may be found in the *Tractatio de instrumentis sciendi* of the *Additamenta*, the treatise preceding that on foreknowledge. Not only did Carbone appropriate Valla's materials, but he rearranged them and ordered them differently, as Valla later complained.[30]

TABLE III

CORRELATIONS FOR MS 27:
GALILEO, CARBONE AND VALLA

No. of Question	Galileo MS 27	Valla-Carbone (1588) *Additamenta* (1597)		Valla *Logica* (1622)
TREATISE ON FOREKNOWLEDGE				
Foreknowledge of principles:				
1	4r2	42ra-42va	c. 8	2:149
2	4v14	40vb-42ra	c. 7	2:147
3	5vl	42va-43ra	c. 9	2:150
4	6r4	43vb-44rb	c. 11	2:150
Foreknowledge of subjects:				
1	6v23	45vb-48ra	cc. 14-16	2.159, 163-165
2	8r19	48ra-49ra	c. 17	2:160
3	9r24	49ra-50ra	c. 18	2:161
4	10r13	50ra-50va	c. 19	2:164
5	10v13	28rb-39ra	c. 4	2:164
Foreknowledge of properties and conclusion:				
1	11r11	45ra-45vb	c. 13	2:156
2	11v25	55rb-56vb	c. 25	2:153
TREATISE ON DEMONSTRATION				
Nature and importance of demonstration:				
1	13r17			2:200
2	14r15	28va-31va	cc. 2,9	2:123, 406-409
Properties of demonstration:				
1	17v17			2:221
2	18v2			2:224
3	19v10			2:229
4	20v6			2:235
5	21r16			2:248
6	22r7			2:250
7	23v5			2:253
8	26vl			2:257
9	27r18			2:266
10	27v23			2:273
11	28r28			2:281
Kinds of demonstration:				
1	29r14			2:299
2	30v20			2:313
3	31r6			2:343

Moreover, when one continues on with the surviving portions of Valla's course on natural philosophy, especially his treatise on the elements, one can verify that Galileo's MSS 27 and 46 both excerpt the essential content of Valla's course work at the Collegio.[31] A good part of Valla's natural philosophy is unfortunately no longer extant, but the missing portions can be supplemented by the notes of his colleagues, Vitelleschi and Rugerio, who essentially duplicated his teachings and thus are adequate for our purposes. Rugerio is particularly helpful in this regard, for his entire course is still conserved in seven volumes of manuscript.[32] These show that, over a three-year period, he gave some 1100 lectures in all, devoting 310 lectures to logic, 500 to the *scientiae naturales*, and 300 more to the *De anima* and the *Metaphysics* — a truly compendious and systematic treatment of the whole of philosophy.

Galileo and Clavius

From these brief indications it is apparent that an extensive body of knowlede, methodological and scientific in the then-accepted sense of science, was being covered each year at the Collegio Romano. Paralleling these "philosophical" investigations, there was also a heavy concentration on mathematics, and here the principal architect of the Collegio program was Clavius himself. Originally from Bamberg, but having studied with Pedro Nuñez at Coimbra before coming to Rome, Clavius was pre-eminent in his field, "the Euclid of the sixteenth century", as he was known.[33] Not only was he concerned with providing the Society of Jesus with men properly trained in pure and applied mathematics, but he was aware that mathematical knowledge is essential for the development of the natural sciences and on this account stressed its importance in the philosophy curriculum also. Pereira, as already noted, had fostered an anti-mathematical attitude in his philosophy courses, following in this the lead of the peripatetics then teaching in the Italian universities. Through Clavius's influence this mentality was overcome, and by the late 1580s and early 1590s mathematical astronomy was being taught concurrently with the *De caelo* and "calculatory" arguments were being discussed in the tracts on the continuum and on elemental bodies.[34]

Galileo's first contact with Clavius came in 1587, during a visit to Rome after having left his studies at the University of Pisa to pursue a career in mathematics. A year earlier he had composed an original treatise, *Theoremata circa centrum gravitatis solidorum*, which he wished to circulate among prominent mathematicians for their critique.[35] Apparently he left a copy of this with Clavius in late 1587, for there is an interchange of correspondence between them

concerning it in 1588.[36] Clavius was very impressed with Galileo's work, and in fact collaborated with Guidobaldo del Monte to secure the young mathematician a teaching position in one of the universities. With regard to the *Theoremata*, however, he had a difficulty: Galileo's logic was not flawless, for it involved a *petitio principii*, i.e., it presupposed the very point it attempted to prove.[37] Because of the coincidence of dates and subject matter — note that this was 1588 and the problem related to the role of *suppositiones* in demonstration, precisely the matter covered in Valla's logic course and finished in that year — it is tempting to look to Clavius as the intermediary through whom Galileo gained access to Valla's lecture notes. There is no mention of this in the correspondence, but the fact that Valla distributed them and that Carbone had secured a set argues for their availability at precisely the time Galileo would have benefited from studying them. If Clavius did Galileo this favor, once Galileo saw the thoroughness with which logical questions were treated at the Collegio, perhaps as contrasted with his own previous instruction, it would have been reasonable for him to seek additional lecture notes on the heavens, the elements, and the local motion of bodies. These, after all, were topics in which he was greatly interested, on whose mathematical treatment he would soon be (or already was) lecturing at the University of Pisa.

In the absence of apodictic proof, this seems about the best way to account for Galileo's acquaintance with the works of the young Jesuits discussed earlier in this essay. And if one peruses carefully their courses in logic and natural philosophy, and then studies Galileo's later compositions — not only MS 71 but most of his treatises down to the *Two New Sciences* of 1638 — one finds unmistakable Jesuit influences in Galileo's work.[38]

Causality and Science

At this point let us return to MS 27 and review some of the teachings in it that are amplified in MSS 46 and 71 and continue to resurface in Galileo's later writings. Being concerned with the *Posterior Analytics*, Galileo's two treatises in MS 27 deal mainly with demonstrative argument and how causes can be used in this type of argument to secure scientific proof. Some idea of Galileo's knowledge of causality and its employment in such argument can be gained by reviewing briefly the distinctions he makes in these Latin manuscripts regarding causes, the maxims and principles that regulate their use, and the ways in which they function in science and demonstration.[39] With regard to causal distinctions, Galileo differentiates between true and proper causes, *verae causae*, and those that are improper and virtual;[40] between universal and particular

causes;[41] between causes *per se* and those *per accidens*;[42] between univocal and equivocal causes;[43] between internal causes, matter and form, and external causes, agent and end;[44] between the four kinds of physical cause — efficient, material, formal, and final — and then between the two subspecies of final cause, intrinsic and extrinsic;[45] between creating and conserving causes;[46] between proximate or immediate causes and those that are remote[47] between causes *in essendo* and those *in cognoscendo*;[48] between causes more known to us and those more known in themselves;[49] between causes convertible with their effects and those that are not;[50] and so on.

Some of Galileo's general maxims are of interest. For example: cause and effect are correlatives;[51] a particular effect must have a particular cause, and so a universal effect must have a universal cause;[52] a positive effect must have a positive cause;[53] and a single effect *per se* must have a single cause *per se*.[54] All of these are equivalent to saying that similar effects must have similar causes. Galileo also intimates that the quantitative variation in an effect will be traceable to a quantitative variation in its cause, as in his noting that resistance increases because the cause of resistance increases.[55]

Other of his principles are more specifically concerned with particular causes, such as God and nature. God he acknowledges as the first efficient and final cause of the universe, thus as the efficient cause of the origin of the elements, and as a supreme cause that can supply for the concursus of any extrinsic cause.[56] Nature is for him a principle of motion, and thus different motions reveal different natures;[57] nature, moreover, does not tend to anything infinite and indeterminate but rather acts for a specific end.[58] A natural cause when not impeded, he writes, produces an effect equal to it in perfection; similarly, any natural cause sufficient to produce its effect functions necessarily given the requisite conditions.[59] In nature a form differs from an efficient cause in that the form exists at the same time as that of which it is the form, whereas an efficient cause, operating as it does through motion, usually precedes its effect in time.[60] Yet Galileo also admits that in natural things it is possible for efficient causes to coexist temporally with their effects.[61] Again, in nature effects are usually more known to us than their causes, where as in mathematics causes are more known both to us and in themselves.[62]

Coupled with these statements, finally, are others that relate more directly to science and demonstration. Since a thing depends on causes for its being, Galileo observes, so also it must be known through them.[63] Science consists in knowledge of the cause that makes a thing be what it is; obviously knowledge that is had through a cause is better than that which is not.[64] Science itself is the effect of demonstration.[65] Sometimes we demonstrate from the final cause, sometimes from the efficient, sometimes from the formal and material, but the more perfect demonstration will proceed from the formal cause since that is more intrinsic to the thing.[66] The two

main types of demonstration are the *propter quid* and the *quia*: the first is made through proximate causes that are true and proper *in essendo*, and these may be either intrinsic or extrinsic; *quia* demonstration, on the other hand, is from a remote cause or from an effect, the latter when the effect is more known to us.[67] This usually happens in the natural sciences, where the demonstrative process involves a resolution and a composition.[68]

The procedure usually followed in the study of nature, Galileo elaborates, is that of the demonstrative *regressus*.[69] This *regressus* is made up of a twofold *progressus* or two *progressiones*; the first *progressus* is from effect to cause and the second from cause to effect.[70] The charge of circularity can be avoided, he points out, because it is one thing to come to know a cause *materialiter* and quite another to come to recognize it *formaliter*, i.e., precisely as it is the cause of a proper effect.[71] The first *progressus* must be a *quia* demonstration that concludes from a more known effect to the existence of an unsuspected cause, which at first is grasped only in a material way. Then, after due consideration of the mind, one sees that the newly discovered cause properly and formally accounts for the effect from which the first *progressus* started; at this point one can proceed to the second *progressus* that makes explicit the *propter quid* explanation of the effect.[72] The *regressus* has almost no place in mathematics, because causes there are more known than their effects; in natural science it is the indispensable way of uncovering the causes operative in nature and then manifesting precisely how they are productive of proper effects.[73]

Influence in Galileo's Scientific Writings

With this rich store of knowledge relating to causes and causal explanation, it is not surprising that Galileo makes constant use of it throughout his scientific writings. In the *De motu antiquiora* drafts of MS 71, for example, he is at pains to distinguish accidental causes from essential causes that affect the motions of bodies.[74] Resistance and other extrinsic impediments he recognizes as accidental causes, and he attempts to minimize them experimentally or else to remove them entirely through the use of appropriate *suppositiones*.[75] He must employ a resolutive method, he writes, to discover the *vera causa that* explains why bodies accelerate when they fall; his adversaries, the peripatetics in the universities, have erred in this because they confuse *causae per accidens* with *causae per se*.[76] The *vera causa* he than proposed, which later he recognized was not really *vera*, was the residual or lightness left in a body after removal from its proper place; as this would be gradually overcome by the body's *gravitas*, he explained, the velocity of fall would correspondingly

increase — again an example of an effect being quantitatively related to the cause producing it.[77]

Galileo's teaching notes on mechanics, *Le meccaniche*, developed between 1593 and 1599, fit into the same mold. The aim of mechanics, he maintains, is to investigate the causes of marvelous effects that seem even to cheat nature in their production. It is a demonstrative science that employs *definitiones* and *suppositiones*, and from these one can uncover causes and provide strict demonstrations of various properties one observes in weights and their movements.[78] Once accidental causes are eliminated, as in the *De motu antiquiora* reasoning, Galileo maintains that he can discern the true principles behind both static and dynamic phenomena. For example, neglecting extraneous and accidental impediments, a weight can be moved by any minimal force over and above the force required to support it.[79] Similarly, a body on a level surface in the plane of the horizon can be moved by any minimal force whatever.[80] The force of percussion is more difficult to analyze, but he announces that he is searching also for the cause of this phenomenon — which at the time of writing still eluded his grasp.

Other teaching notes, in Italian, that survive from Galileo's professorship at Padua include his *Trattato della Sfera*, or treatise on the *Sphere* of Sacrobosco, which dates from the early 1600s. In this he presents the essentials of the Ptolemaic system in the form of a *scientia media*, with appropriate suppositions and demonstrations from which the appearances of the heavens can be calculated and presented in tabular form.[81] The demonstrations in this work are all geometrical, and the treatise itself can be shown to be heavily dependent on the more extensive exposition in Clavius's *Sphaera*, the second edition of which (1581) Galileo used in the writing of MS 46. There are no references to physical causes in the *Trattato*, nor should one expect there to be, since Galileo is explicit that he is using certain properties of circles and straight lines (i.e., properties that flow from formal causes) to calculate the positions of the heavenly bodies.[82] During this same period, however, we now know that he was very much concerned with the physical causes of the motions of bodies down inclines, in free fall, and when suspended in various pendular arrangements. His experimental discoveries prior to 1609, in fact, provided him with most of the materials on which the *Two New Sciences* of 1638 would be based, which will be discussed later in this essay.

The year 1609 was momentous in its own right, for at the end of that year Galileo made his discoveries with the telescope that were profoundly to affect the course of his life. In the *Sidereus nuncius* of 1610 he called attention to numerous new effects in the heavens that would resist explanation in terms of accepted notions and would set him on the search for their causes — causes previously unknown throughout the entire history of mankind. His claims were quickly

challenged, so it is not surprising that his writings from this time onward took on a strong rhetorical and polemical tone that makes it difficult for one to disengage in them reasoning that is demonstrative from that which is merely dialectical.

The *Discourse on Floating Bodies* of 1612 is Galileo's first work of this genre, directed against Ludovico delle Colombe and other conservative Aristotelians of Florence. But in it there is no difficulty discerning Galileo's true aim: to find the true, intrinsic, and total cause of flotation.[83] Considering all of the phenomena and experiments that have been excogitated, he clearly states that he will reduce the causes of such effects to their more intrinsic and immediate principles, since this in required by the *progressio dimostrativa.*[84] Note Galileo's use of this expression, the Italian equivalent of that in MS 27, where he makes repeated use of *progressus* and *progressio* to explain the *regressus demonstrativa* and how it must be employed in the physical sciences.[85] Many causes will affect a body's motion and rest in water, he says, but for him the important thing is to determine the proximate and immediate cause of these effects.[86] A cause, he now explains, is that which, being present, the effect is there, and being removed, the effect is taken away.[87] Using this as a criterion, he feels that he has successfully determined the true, natural, and primary cause of a body's floating or sinking, namely, its specific gravity relative to that of the medium in which it is immersed.[88] Galileo's analysis displeased the peripatetics of Florence, but it was accepted as a brilliant work by G. Biancani (Biancanus), a Jesuit mathematical physicist trained by Clavius, who published only a few years later an erudite treatise explaining how causes of various kinds are employed in all branches of pure and applied mathematics.[89]

More directly related to the discoveries with the telescope are Galileo's *Letters on Sunspots*, his *Letter to Christina*, and his controversy with Orazio Grassi over the nature and movement of comets. Both the sunspot and the comet disputes were with Jesuits who were working in the same areas as Galileo and who shared with him a common terminology. Thus we should not expect in them any repudiation of causal analysis. His disagreement with the German Jesuit Christoph Scheiner over the motion of sunspots arose precisely because Galileo was convinced that sunspots were defects in the surface of the sun whose movement was caused by the sun's rotation — a causal argument if ever there was one. His argument with Grassi, an Italian Jesuit then teaching at the Collegio Romano, was on a different basis. Grassi was convinced that the comets of 1618 were real objects whose parallax measurements showed them to be far above the orb of the moon, whereas Galileo, afraid that the path claimed by Grassi for the comets might count against the Copernican hypothesis, held that they were not real objects but merely optical illusions.[90] Admittedly such a dispute was not about causes directly, and yet it involved them indirectly. The entire debate hinged on

whether the appearances observed through telescopes were caused by something moving beyond the sphere of the moon or by some aberration within the lenses themselves. In either event the argument was about the true cause of the images being studied, clearly an instance of causal reasoning.

Copernican Controversies

The more important writings of 1615-1616 were concerned with the truth of the Copernican system itself and how this system could be reconciled with statements of Sacred Scripture. Now Galileo's letter of 1615 to the Grand Duchess Christina, mother of his patron Cosimo II de Medici, is filled with assertions that the earth's motion, both diurnal and annual, can be conclusively proved by necessary demonstrations based on sensate experiences.[91] Galileo's terminology is undoubtedly that of the *Posterior Analytics*, though his letter to Christina is remarkable in that it does not outline a single demonstration that proves either component of the earth's motion. Earlier in 1615, however, Cardinal Bellarmine had written to Paolo Foscarini, a Carmelite friar, commending him and Galileo for not claiming that their Copernican proof was apodictic, since it was merely argued *ex suppositione*. In his reflections on Bellarmine's letter, which are recorded in the National Edition of his works, Galileo takes a position, consistent with the logic notes of MS 27, wherein he defends the possibility of a strict demonstration that makes use of *suppositiones*. It can do so, he maintains, provided that the suppositions are true in nature and not merely fictive hypotheses arbitrarily concocted to save one or other appearance.[92] In MS 27 he had written that principles of demonstration need not be *per se nota* on their own terms; they could also be shown to be true by *a posteriori* argument.[93] Late in 1615, therefore, Galileo must have felt that he had a conclusive effect-to-cause proof of the earth's motion. The sketch of such a proof is indeed to be found in Galileo's letter to Cardinal Orsini of 8 January 1616, containing his *Discorso del flusso e reflusso del mare*, which lays the groundwork for the causal analysis of the tides that would later bring to conclusion the celebrated *Dialogue* of 1632.

The marvelous problem he is addressing, Galileo writes to Orsini, is that of finding the true cause, the *causa vera*, of the ebb and flow of the sea, hidden and difficult to discover, but now fortunately laid bare by him.[94] This true cause readily and clearly explains all the effects and properties of the ocean's motions. Because of the complexity of these movements, it is necessary to assign a primary cause for them and then to add other secondary and concomitant causes to account for their diversity.[95] The principal cause, on

further consideration, turns out to be twofold. The first component is the alternate acceleration and deceleration of the earth produced by the composition of its two motions, diurnal and annual. These add to and subtract from each other and so cause the waters of the ocean to slosh back and forth in a twenty-four hour cycle. The other cause depends on the *propria gravità* of sea water, which alters the primary motion in various ways depending on the dimension of the sea bed in which the water moves. This has the additional effect of producing periods or cycles of various durations in different parts of the world. Such seems to Galileo to be the *causa adaequata* of tidal effects, and on its basis he is not proposing the Copernican system as a mere fictitious hypothesis but rather as based on principles that reflect the true structure of the universe.[96]

The causal reasoning outlined in this preliminary *Discorso* was not a passing fancy with Galileo, for he continued to work on it and perfect it for some fifteen years. The accession of Matteo Cardinal Barberini to the throne of Peter as Pope Urban VIII in 1623 gave him the opportunity he sought, and after much effort he had the expanded version of the *Discorso*, now cast in the form of a *Dialogue* on the two chief world systems, ready for publication. In its final version of 1632 the *Dialogue* occupies four days of discussion, the first of which aims to destroy the previously accepted dichotomy between the terrestrial and the celestial regions. The second day examines all the arguments brought against the earth's daily rotation on its axis, and shows that these lead to the same result whether the earth is moving or at rest. The third day exposes the weaker evidence Galileo has at hand to support the earth's being a planet and making a great annual orbit around the sun. Since none of the arguments of the second and third day is apodictic, and indeed none is proposed as absolutely conclusive by Galileo, he feels constrained to add — and this contrary to the advice of his Roman censor, the Dominican Niccolò Riccardi — a fourth day devoted to the tidal argument.

There is little point in reviewing the details of that argument or analyzing its defects.[97] For purposes here it suffices to observe that the same causal distinctions and maxims that were earlier noted resurface throughout the discussions of the fourth day. When seeking an explanation of the tides, Galileo writes, one must first identify the principal effects, and from these one can proceed to discover the true and primary causes.[98] Effects that are similar in kind must be reducible to a single, true, and primary cause. Indeed, there is only one true and primary cause for any one effect.[99] Not only this, but there is a fixed and constant connection between cause and effect, so that any alteration in the one will be accompanied by a fixed and constant alteration in the other.[100] Apart from the principal effects there will also be accidental variations, but these are reducible to different accompanying causes, secondary or accidental

causes associated in some way with the operation of the primary cause. This is obviously the same terminology as employed in the *Discourse on Floating Bodies* and the Latin manuscripts dating from Galileo's teaching days at the University of Pisa.

Coming finally to the *Two New Sciences* of 1638, we noted earlier that this last and most famous work of Galileo puts in synthetic form the results of his experimental studies of mechanics and motion completed before 1610, the year of publication of the *Sidereus nuncius*. The work is mathematical in character, much more so than the *Dialogue* of 1632, and this affects the degree to which it makes reference to physical causes. Yet such references are far from absent. The main problem Galileo addresses in his mechanics is that of accounting for the strength of materials under various types of stress. To solve this problem he has recourse to his pervasive axiom: for any one effect there must be a single, true, and optimal cause.[101] It is somewhat embarrassing for some to discover that the true cause he identifies for the cohesive effects of materials under stress is the minute vacua spread throughout their substance that resist separation because nature abhors a vacuum. His analysis of motion is more successful, but here he is less precipitate in pointing out the *vera causa* of a body's changing velocity during fall than he was in the *De motu* contained in MS 71. Now he recognizes that the motion of such bodies is uniformly accelerated with respect to time, and he explains this in terms of nature's uniform action on the body, conferring on it equal increments of velocity in equal intervals of time. What is perhaps most interesting here is his recognition that the cause of any natural motion is internal, recalling his memoranda in the latter part of MS 46 wherein he cites Aristotle to the effect "that for naturalness of motion an internal, not an external, cause... is required."[102] Galileo's unwillingness here to discuss other causal possibilities, which some interpret as his definitive rejection of causal inquiries, was therefore no such thing. What he was rejecting was his own previous identification of the *vera causa* of velocity change, which he had found to be erroneous. Before one can specify such a cause, he now insisted, one must first demonstrate the properties of the accelerated motion under investigation. The causal ideal of scientific explanation was still very much Galileo's own. That is why he could confidently assert toward the end of the *Two New Sciences* that the knowledge of a single effect acquired through its causes opens the mind to the certification of other effects, even without recourse to experiments.[103]

There seems little doubt, from this survey, that Galileo consistently employed causal argument over the fifty-year period extending from 1588 to 1638. The problem of causality in his science is clearly not whether he sought causal explanations, but rather how he sought them and how he thought they could lead to certain and unrevisable knowledge about the physical world. And the answer to this how

question is contained in germ in Galileo's very first Latin composition, MS 27, in the two concepts already noted, *suppositio* and *regressus*, as required to achieve strict demonstrations in the science of nature. The evolution of these concepts, and their adaptation by Galileo to accomodate the mathematical and experimental modes of investigation in which he pioneered, can serve to explain his success with the *nuova scienza* that has become the prototype for the mathematical physics of the present day. [104]

Reinterpreting Galileo

The redating and renewed study of Galileo's Latin manuscripts cannot help but inaugurate a revisionist movement among those interpreting the great Italian's writings and assigning them their proper place in the histories of science and philosophy. To attempt even a brief outline of possible consequences of such efforts would take us far beyond the compass of this essay. By way of conclusion, however, it may not be amiss to dispel a few myths that surround this pioneering astronomer-physicist, which can no longer hold up in light of the historical evidence that has now been unearthed.

One of the most famous legends, to be sure, is that of the Leaning Tower of Pisa, wherein it is believed that Galileo obtained conclusive experimental proof of his law of falling bodies while he was teaching at the University there. A close study of MS 71 and its reference to the experiments of Girolamo Borri (which are also referenced in the Jesuit lecture notes, by the way) reveals what actually happened around that time, i.e., in 1590 or 1591. [105] Galileo probably did drop objects from the Leaning Tower, but during his residence in Pisa he was not yet in possession of the law of falling bodies. His tests were of the kind then being used to see whether a wooden object or a leaden object would fall faster in air — a matter of dispute related to the problem of whether air has weight in air. Since such evidence was then being discussed and evaluated at the Collegio Romano and elsewhere, there is nothing strikingly original about this alleged episode in the life of Galileo. [106]

Related to this is a general myth about the stark originality of Galileo's scientific thought. Historians of science following the lead of the positivist Ernst Mach have fostered the view of a sharp discontinuity between late medieval and early modern science. Galileo they see as a kind of Melchisedech without forebears, whose university training was worthless, and who rejected everything that his teachers had taught him. Particularly significant, for them, was his spurning of Aristotle and the Aristotelian ideal of causal explanation. In their reading Galileo would have nothing to do with causes, but turned instead to mathematics and experiment for the sole

certification of his scientific method, which they tend to identify with the hypothetico-deductive method of twentieth-century science. The fact of the matter is that Galileo was a man of his times who was well acquainted with the thought of progressive Aristotelians such as the Jesuits and who made good use of causal analysis and the methodological canons of the *Posterior Analytics*. Indeed, following the guidelines early laid out in MS 27, he attempted to formulate his new *scientia* with the aid of *principia, definitiones, suppositiones,* and *demonstrationes* so as to furnish strict proof of the *proprietates* and *passiones* he attributed to his proper subject. It is true that he manifested great originality in devising experiments and developing mathematical techniques, particularly those dealing with proportionalities and limit concepts. But all of this was done in an Aristotelian-Euclidean-Archimedean context that, as it turns out, is quite foreign to the thought of twentieth-century empiricists.

Of more profound significance is the bearing of these new findings on the understanding of the Trial of 1633 and the book that occasioned it, the *Dialogue* of 1632. There are many legends that have grown up around the trial and Galileo's disastrous encounter with the Roman Inquisition. In the popular mind, for example, it is thought that Galileo offered conclusive proof of the Copernican system and that he was forced to perjure himself by the Inquisition in swearing that the earth stands still. Before the transcription of the logical questions in MS 27, and the discovery of the source from which they derive, one might have wondered about Galileo's knowledge of demonstration and of the canons of proof that would be required to justify a claim of the earth's motion. Now that this * information is available, it is quite clear that the had a sophisticated awareness of the problem. On rereading the *Dialogue* in the light of MS 27, in fact, one is impressed that nowhere in the four days of its discussions does Galileo claim to have *demonstrated* the earth's movement, although in many of his writings leading up to this work he had made other demonstrative claims, and of course the *Two New Sciences* of 1638 is replete with them. Recent analyses of the *Dialogue* serve to strengthen this view, for they portray this as a rhetorical work aimed at urging the acceptance of the Copernican system in the absence of conclusive proof.[107] If this was Galileo's real intention, and he himself was aware that the truth of the Copernican theory had not yet been established, then he would not have perjured himself when assenting to the Church's interpretation of the Scriptural passages that argue against the earth's motion. He was simply accepting on faith that the earth does not move, which he could do in clear conscience if his reason had failed to prove the opposite.[108]

These, then, are a few of the common impressions about Galileo that stand to be corrected as the result of recent scholarship. It goes without saying that such ceding of legend to fact takes nothing away

from the genius of this founder of modern science. To be aware of a long and venerable tradition, to recognize its limitations, and then to transcend it with a highly original program of research and investigation is one of the loftiest accomplishments of the scientific mind. But no less important was Galileo's striking testimony, written large in the events of 1633 and their aftermath, that being a scientist need not preclude one's having a strong religoius faith, and that is surely a potent lesson for our time.

REFERENCES

[1] These manuscripts are preserved in the Galileiana collection of the Biblioteca Nazionale Centrale in Florence; the numbers are those of the codices there in the Galileo font. The present numbering of the manuscripts unfortunately does not correspond to that in the National Edition referenced in the following note. A useful guide to all the sources of that edition, which provides both sets of numbers, is Eugenia Levi, *Indice delle Fonti dell'Edizione Nazionale delle Opere di Galileo Galilei*, (G. Barbèra Editore, Florence, 1968).

[2] Antonio Favaro, ed., *Le Opere di Galileo Galilei*, 20 vols. in 21, (G. Barbèra Editore, Florence (1890-1909), reprinted 1968, Vol. 9, p. 279, and Vol. 1, p. 12, henceforth referenced as *Opere* 9:279 and *Opere* 1:12, or simply as 9:279 and 1:12 when it is clear from the context that the reference is to the National Edition.

[3] *Opere* 1:7-177.

[4] *Opere* 9:273; the list of questions the manuscript contains is given on 9:280-281, and samples of the text on 9:279 and 9:291-292.

[5] *Opere* 1:234, 249; for the transcription itself, see 1:251-408.

[6] The pioneer work here was a doctoral dissertation completed at the University of Montreal by Fredette, a portion of which has been published as "Galileo's *De motu antiquiora*" in *Physis*, 14 (1972), pp. 321-348. The same author contributed an important paper, as yet unpublished, to the 1975 Workshop on Galileo at Blacksburg, Virginia, entitled "Bringing to Light the Order of Composition of Galileo Galilei's *De motu antiquiora*."

[7] The fuller titles are *Galileo's Early Notebooks: The Physical Questions, A Translation from the Latin, with Historical and Paleographical Commentary*: (University of Notre Dame Press, Notre Dame, 1977), and *Prelude to Galileo: Medieval and Sixteenth-Century Sources of Galileo's Thought,* Boston Studies in the Philosophy of Science 62 (D. Reidel Publishing Company, Dordrecht-Boston, 1981).

[8] *Opere* 1:9.

[9] The manuscript has been transcribed by W. F. Edwards of Emory University and independently by Adriano Carugo of the University of Venice, neither of whom has thus far published his reading of it. The excerpts cited below are based on Edwards' transcription, with his permission; they have been verified by the author against the original.

[10] Portions of that analysis are provided below; for a fuller account see the author's *Galileo and His Sources: The Heritage of the Collegio Romano in Galileo's Science* (Princeton University Press, Princeton, 1984), pp. 3-53, 89-95.

[11] Substantiation of this statement is the burden of the work cited in the preceding note.

[12] For a documented history of the Collegio, see R. G. Villoslada, "Storia del Collegio Romano dal suo inizio (1551) alla soppressione della Compagnia di Gesù (1773), *Analecta Gregoriana* 66 (Gregorian University Press, Rome, 1954).

[13] Bibliographical details are given in *Galileo's Early Notebooks,* pp. 13-14, and in *Prelude to Galileo,* pp. 196-200, 207-208.

[14] Most of these manuscripts are described in *Galileo's Early Notebooks,*

pp. 13, 307-309; a few additional ones have been added in *Galileo and His Sources,* pp. 351-354, which contains full descriptions of all Jesuit *reportationes* used in this study.

[15] Bibliographical details on Menu are sparse; see C. H. Lohr, "Renaissance Latin Aristotle Commentaries", *Renaissance Quarterly,* 31 (1978), p. 583, and the indices to *Galileo's Early Notebooks, Prelude to Galileo,* and *Galileo and His Sources* for references to Menu's teachings.

[16] A *rotulus* of professors and the courses they taught in various years at the Collegio, as far as these are known, has been prepared by Ignazio Iparraguirre and is appended to Villoslada's *Storia del Collegio Romano,* pp. 321-336. The listing in Table I is based on this *rotulus* but is supplemented with additional information unearthed by the author in his manuscript searches.

[17] The notes are conserved in Cod. 138 (A.D. 1577-1579), Leopold-Sophien-Bibliothek, Ueberlingen, West Germany.

[18] Information on Valla is also sparse; see Carlos Sommervogel *et al., Bibliothèque de la Compagnie de Jésus,* 11 vols., Brussels-Paris: Alphonse Picard, 1890-1932, Vol. 8, col. 418, as well as the indices to *Galileo's Early Notebooks, Prelude to Galileo,* and *Galileo and His Sources* for references to Valla's teachings.

[19] Vitelleschi's manuscripts are listed in *Galileo's Early Notebooks,* p. 308, and *Galileo and His Sources,* pp. 353-354; for Rugerio's manuscripts, see note 32 below.

[20] This evidence is provided in Part I of *Galileo and His Sources.*

[21] The basic account is given in *Galileo and His Sources,* Chap. 1, but some additional details are to be found in the author's "Galileo's Sources: Manuscripts or Printed Works?", in *Print and Culture in the Renaissance,* eds. Sylvia Wagonheim and Gerald Tyson (University of Delaware Press, Newark, Delaware, forthcoming).

[22] The contents of the manuscript are listed in Table 3 of *Galileo and His Sources,* pp. 30-32; a brief summary is given in Table III below.

[23] The full title is *Additamenta ad commentaria D. Francisci Toleti in Logicam Aristotelis. Praeludia in libros Priores Analyticos; Tractatio de Syllogismo; de Instrumentis sciendi; et de Praecognitionibus, atque Praecognitis.* Auctore Ludovico Carbone a Costacciaro, Academico Parthenio, et in Almo Gymnasio Perusino olim publico Magistro. Cum Privilegiis. Venetiis: Apud Georgium Angelerium, 1597. This is cited in what follows as *Additamenta.*

[24] The title reads: *Logica Pauli Vallii Societatis Iesu duobus tomis distincta: Quorum primus artem veterem, secundus novam comprehendit.* Henceforth it is cited as *Logica.*

[25] *Logica,* Vol. 2, fol. 1. The Latin text is given in *Galileo and His Sources,* p. 19; the English translation is the author's.

[26] Many of these parallels are exhibited in Chap. 1 of *Galileo and His Sources,* which should be consulted to grasp the full import of this statement.

[27] MS 27, fol. 7r; the insert occurs at line 27. In what follows, line numbers are added directly after the foliation, and thus a reference of this type reads MS 27, fol. 7r27, or simply MS 27, 7r27.

[28] *Additamenta,* fols. 46v-47r.

[29] Valla-Carbone's text is reproduced from *Additamenta,* fols. 46v-47r, Galileo's from MS 27, fol. 7r24-v7.

[30] See the text cited above at note 25.

[31] Apart from the documentation provided in Part I of *Galileo and His Sources,* details are given in the commentary section of *Galileo's Early Note-*

books, especially pp. 281-303, and in Part III of *Prelude to Galileo.*

[32] These bear the signature Msc. Class. Cod. 62-1 through 62-7, Staatsbibliothek, Bamberg, West Germany.

[33] For brief biographical and bibliographical information about Clavius, see Sommervogel, *Bibliothèque,* Vol, 2, cols. 1212-1224.

[34] *Prelude to Galileo,* p. 231, and *Galileo and His Sources,* pp. 136-141; additional details will be found in Giuseppe Cosentino, "L'insegnamento delle matematiche nei collegi Gesuitici nell'Italia settentrionale: Nota introduttiva," *Physis,* 13 (1971), pp. 205-217; *idem,* "Le matematiche nella 'Ratio Studiorum' della Compagnia di Gesù", in *Miscellanea Storica Ligure,* 2.2 (1970), pp. 171-123; and A. C. Crombie, "Mathematics and Platonism in the Sixteenth-Century Italian Universities and in Jesuit Educational Policy," in *Prismata: Naturwissenschaftsgeschichtliche Studien* (Festschrift für Willy Hartner), Y. Maeyama and W. G. Saltzer, eds,. pp. 63-94 (Franz Steiner, Wiesbaden, 1977).

[35] The treatise is transcribed in *Opere* 1:179-208.

[36] See *Opere* 10:22-24, 27-29.

[37] *Opere* 10:24, line 4, henceforth written as 10:24.14.

[38] Fullest treatment of these influences is given in *Galileo and His Sources;* their main features are outlined in the author's "Aristotelian Influences on Galileo's Thought," in *Aristotelismo Veneto e Scienza Moderna,* ed. Luigi Olivieri, 2 vols. (Editrice Antenore, Padua, 1983), Vol. I, pp. 349-378, followed by an Italian translation of the same, pp. 379-403.

[39] Materials in the remainder of this essay are abbreviated from the author's "The Problem of Causality in Galileo's Science," in the *Review of Metaphysics,* 36 (1983), pp. 607-632, which should be consulted for the relevant excepts (in Latin and Italian) from Galileo's writings.

[40] MS 27, 18v9-10, 19r3-4, 28v10.

[41] MS 46, 1:25.12-13.

[42] MS 46, 1:166.34-36; MS 71, 1:266.8-9, 1:307.12-14, 1:317.13-14.

[43] MS 46, 1:128.10-12, 1:165.19-21.

[44] MS 27, 30v17-18.

[45] MS 27, 13r31-vl; MS 46, 1:129.3-5.

[46] MS 46, 1:35.13-17.

[47] MS 27, 19r31-32, 30v38-39.

[48] MS 27, 29v31-33.

[49] *Ibid.*

[50] MS 27, 30v17-19, 31v18-19.

[51] MS 27, 5v9-11.

[52] MS 46, 1:25.12-13.

[53] MS 46, 1:161.22, 1:416.4.

[54] MS 46, 1:164. 26-27.

[55] MS 46, 1:172.9-11.

[56] MS 46, 1:24.30-25.1, 1:25.10-12, 1:128.6-7, 1:146.4.

[57] MS 46, 1:57.33-58.1.

[58] MS 46, 1:149.19-21.

[59] MS 27, 22v14-15, 12v24-25.

[60] MS 46, 1:32.30-32, 1:33.1-4.

[61] MS 46, 1:32.25-27.

[62] MS 27, 22v1-2, 29v31-32.

[63] MS 27, 5v7-9.

[64] MS 27, 18v31-32, 22v9-10.

[65] MS 27, 18v31.

[66] MS 27, 18v30-33.
[67] MS 27, 9v17-19, 13r21-22, 30r21, 30r25-27.
[68] MS 27, 14r30.
[69] MS 27, 31r6.
[70] MS 27, 31v7-8.
[71] MS 27, 31r31, 31v16-18.
[72] MS 27, 31v11-12.
[73] MS 27, 31v3-6.
[74] MS 71, 1:266.8-9.
[75] MS 71, 1:302.8-9.
[76] MS 71, 1:318.3-4, 1:317.13-14.
[77] MS 71, 1:322.23-26.
[78] MS 72, 2:155.18-19, 2:159.5-10.
[79] MS 72, 2:179.24-180.6.
[80] MS 72, 2:180.7-10.
[81] MS 47, 2:211.14-212.18. For a summary of his arguments and how these may be reconciled with his incipient commitment to the Copernican system, see the author's "Galileo's Early Arguments for Geocentrism and His Later Rejection of Them," in *Novità Celesti e Crisi del Sapere,* ed. Paolo Galluzzi (Istituto e Museo di Storia della Scienza, Florence, forthcoming).
[82] MS 47, 2:211.14-212.18.
[83] 4:67.5.
[84] 4:67.18-23.
[85] See the texts cited at notes 69-73 above.
[86] 4:86.8.
[87] 4:112.21-23.
[88] 4:120.25-26.
[89] *De mathematicarum natura dissertatio una cum clarorum mathematicorum chronologia,* Bologna: Apud Bartholomaeum Cochium, 1615, pp. 62-64; but see also pp. 13, 19.
[90] For fuller particulars, see W. R. Shea, *Galileo's Intellectual Revolution: The Middle Period, 1610-1632* (Science History Publications, New York, 1972) Chaps. 3 and 4.
[91] For example, 5:312.27-28. Analyses of such assertions and the contexts in which Galileo uses them are to be found in Jean Dietz Moss, "Galileo's *Letter to Christina:* Some Rhetorical Considerations," *Renaissance Quarterly,* 36 (1983), pp. 547-576, especially pp. 562-570 and notes 34, 37.
[92] 5:357-359.
[93] MS 27, 6r3-v22.
[94] 5:377.11-18.
[95] 5:378.18-23.
[96] 5:381.9-10.
[97] On this, see M. G. Galli, "L'argomentazione di Galileo in favore del sistema copernicano dedotta dal fenomeno delle maree", *Angelicum,* 60 (1983), pp. 386-427.
[98] 7:443.25-444.1.
[99] 7:444.2-14.
[100] 7:471.7-11.
[101] 8:66.1-10.
[102] MS 46, 1:416.21-22.
[103] 8:296.20-24.
[104] A provisional reconstruction of how this adaptation took place in

Galileo's developing thought is sketched in *Galileo and His Sources,* pp. 339-347.

[105] MS 71, 1:333-334: see also *Prelude to Galileo,* pp. 116, 248, 250, and 313.

[106] For a detailed account, see Thomas B. Settle, "Galileo and Early Experimentation", forthcoming.

[107] See M. A. Finnochiaro, *Galileo and the Art of Reasoning: Rhetorical Foundations of Logic and Scientific Method* (D. Reidel Publishing Company, Dordrecht-Boston 1980) also Jean Dietz Moss, "Galileo's *Letter to Christina,*" cited in note 91 above; *idem,* "Galileo's Rhetorical Strategies in Defense of Copernicanism," in *Novità Celesti e Crisi del Sapere,* ed. Paolo Galluzzi (Istituto e Museo di Storia della Scienza Florence, forthcoming and *idem*, "The Rhetoric of Proof in Galileo's Writings," *infra, pp. 41-65.*

[108] The author further develops this theme in his "Galileo's Science and the Trial of 1633," *The Wilson Quarterly,* 7 (1983), pp. 154-164, and in an expanded and documented version of the same, "Galileo and Aristotle in the *Dialogo,*" *Angelicum,* 60 (1983), pp. 311-332.

X

THE DATING AND SIGNIFICANCE OF GALILEO'S PISAN MANUSCRIPTS

Among Professor Drake's many contributions to our knowledge of Galileo and his work, the research he has done on the watermarks discernible in Galileo's surviving manuscripts must surely rank the most significant. It was this line of attack that enabled him to date the large number of fragments long known to be associated with the second science set out in Galileo's *Two New Sciences* of 1638, namely, that devoted to *De motu locali*, which in turn led to two important discoveries. The first was chronological, for it showed that the great bulk of these fragments date from Galileo's early period, before he made his momentous discoveries with the telescope. The second was methodological, for it gave nearly indisputable evidence of an experimental program in which Galileo was engaged while writing the fragments, mainly at Padua and prior to 1610. Such a program, undertaken early in Galileo's life, reveals a strong empiricist strain in his thought and counts heavily against his ever being a Platonist, as alleged by some Galileo scholars.[1]

The manifest success of this study of watermarks has encouraged Drake to delve further into Galileo's pre-Paduan writings and to attempt to date them using similar techniques.[2] In that enterprise he takes as a reference point the dating I established for MS 27 (i.e., about 1589), and with that as a keystone constructs a chronological arch extending back to 1584 and forward to 1591, to which he attaches at various points the writing of four other manuscripts or portions thereof. I shall not be concerned with all of these in what follows, but only with MSS 27, 46, and 71, since these are the manuscripts with which I am most familiar and on which I have done the most extensive research.[3] On the basis of their relationships to lecture notes from the Collegio Romano, most of which are dated, I have argued that the three manuscripts were written roughly between 1589 and 1591, while Galileo was at Pisa. Drake's arguments based on watermarks, while reinforcing my results for MS 27, yield conclusions for the two other MSS that vary from mine. With regard to MS 46, for example, he isolates three different components within it and situates their composi-

tion respectively in Pisa (1584), Florence-Pisa (1587—1590), and Florence (1588). With regard to MS 71, on the other hand, he identifies four components, but similarly situates their composition in Siena (1586—1587), Florence (1588), Pisa (1590), and Pisa-Florence (1590—1591).[4] Although Drake provides other evidence, his main support for these locations and times is the type of watermark found on paper produced in the cities indicated. He is aware, no doubt, of the fragility of such claims, but feels that they are sufficiently well founded to serve "as a basis for further research by specialists".[5]

Drake communicated his preliminary results to me before publishing them and we briefly entertained the idea of a joint paper on the early manuscripts.[6] As I wrote to him at the time, the materials I published in *Galileo and His Sources* relating to them represented but a small fraction of what I then had available. This notwithstanding, I conceded that my dating of MSS 46 and 71 in *Galileo and His Sources* was far from definitive and that further evidence, such as that deriving from watermarks, would have to be taken into account. The pressure of other work has prevented me from returning to the project until now, when the prospect of a *Festschrift* honoring him provides me with an ideal opportunity to do so. Meanwhile I have published a number of related studies that corroborate to some extent my earlier findings.[7] Even so, I should warn the reader that, at the present state of the evidence, little certitude can be expected on the subject of this essay. Much data will be adduced, some of it perhaps tedious, with the promise of only a slight yield in knowledge as the final result.

LECTURE COURSES AT THE COLLEGIO ROMANO

One important feature serves to differentiate Galileo's MS 71, largely concerned with treatises *De motu*, from his MSS 27 and 46, concerned mainly with traditional course work in logic and natural philosophy. MS 71 gives evidence throughout of being an original composition, revised and even recopied in places, but all written by Galileo in his own hand. MSS 27 and 46, on the other hand, while also being autographs, show numerous signs of copying. These signs are not those might expect from oral transmission, as, for example, if someone were lecturing or dictating their contents to Galileo.[8] Rather they are typically of the kind that might arise if Galileo were appropriating and abbreviating material from an exemplar, especially one with sufficient abbreviation (as in our

present-day shorthand) to make it difficult to decipher.[9] Signs of copying of this type rule out printed matter as Galileo's source, and point to manuscripts or *reportationes* of lectures as his likely exemplar.[10] Acting on this insight, around 1972 I began to note resemblances between Galileo's MS 46 and lecture notes for the course in natural philosophy at the Collegio Romano, and shortly thereafter noted similar correspondences between his MS 27 and the logic course at the same institution. These findings started me on a program of research that continues to the present day.

In essence this program turns on the fact that the *rotulus* of professors at the Collegio Romano, indicating their subjects and the years in which they taught, survives to the present. Apparently it was the custom for each professor to deposit a set of his lectures in the Collegio's library; some of these were sent to other institutions, usually Jesuit, to serve as models there, and yet others were copied and recopied for various purposes. Only a small number of these are extant, but fortunately enough of them from the period around 1590 are available to permit a reasonably accurate dating of Galileo's compositions.[11]

At that time the course of studies at the Collegio was clearly prescribed, and a fairly standard syllabus was being taught in each of the subjects. The subjects themselves were arranged in a three-year cycle and followed, in the main, the text of Aristotle. The first year was devoted to logic as set forth in the *Organon* and concluded with a detailed study of the *Posterior Analytics*; the second year focused on natural philosophy, covering the *Physics*, the *De caelo*, and the *Meteorology*; and the third year, after concluding the study of natural philosophy with the *De generatione*, treated the *Metaphysics* and the *De anima* to complete the cycle.[12] Usually a professor would begin with a class in the first year and then take that class through all three years of the cycle. Occasionally, however, a professor would manifest particular competence in logic or natural philosophy, say, and would be assigned to teach that specialty more than once. As we shall see, Ioannes Lorinus filled that function in logic and Antonius Menu likewise in natural philosophy.[13] Both left rather complete sets of notes, which apparently were used by their successors to map out their own lectures. Some selectivity and reordering of the materials is detectable from year to year — reflecting, in my view, the varying pedagogical abilities of the lecturers — and yet a remarkable uniformity characterizes the teaching as a whole. The resulting repetition of titles and subtitles into which the

various courses were divided makes it difficult to identify any one professor's notes as Galileo's source, but a careful study of the wording can reveal varying degrees of similarity and other clues that point to notes dating from a particular year as Galileo's likely exemplar.

One professor, though probably not himself Galileo's source, deserves special mention here for the thoroughness of his lectures throughout the three-year cycle and for the fact that he meticulously numbered and dated all of his lectures in the margins of his teaching notes. This is Ludovicus Rugerius, a Florentine, who began the cycle in 1590 and concluded it in 1592, delivering a total of 1088 lectures in the process. All of these lectures are preserved in a series of codices now in the Staatsbibliothek Bamberg.[14] I suspect that they were sent to the Jesuit college there at the behest of Christopher Clavius, then a colleague of Rugerius, who was originally from Bamberg and who wished to provide his fellow Jesuits there with a model teaching program. The course of Rugerius's teaching is plotted in Figure 1. The circled points designate the dates, recorded in his notes, on which he ended one tract of a course and began another; many of these correspond to his finishing a commentary on one of Aristotle's works or on a book within a particular work. The cumulative number of lectures is plotted along the ordinate, and the total number of lectures on a particular work is shown in parentheses under its title. The abscissa, on the other hand, is divided into months, from November of one year to October of the next.

Since MSS 27 and 46 are our main concern, we should note that their contents correspond to only a very small portion of the materials covered each year in these lectures at the Collegio Romano. One of the standard divisions for each course was the treatise, and it is noteworthy that MS 27 contains only two treatises, the first of which may be incomplete, whereas MS 46 contains three treatises, more or less complete, and a fragment of a fourth, much of which has clearly been lost. The two treatises in MS 27 derive from the portion of the course dealing with Aristotle's *Posterior Analytics*, whereas the first two treatises of MS 46 correspond to the matter covered in Aristotle's *De caelo*, the second two to the matter covered in his *De generatione*. Since we may presume that professors worked their way through the course at about the same rate as Rugerius, the chronology of his lectures proves useful for dating when these treatises might have been completed in a particular year and thus indicate the earliest time at which they could have been available to Galileo.

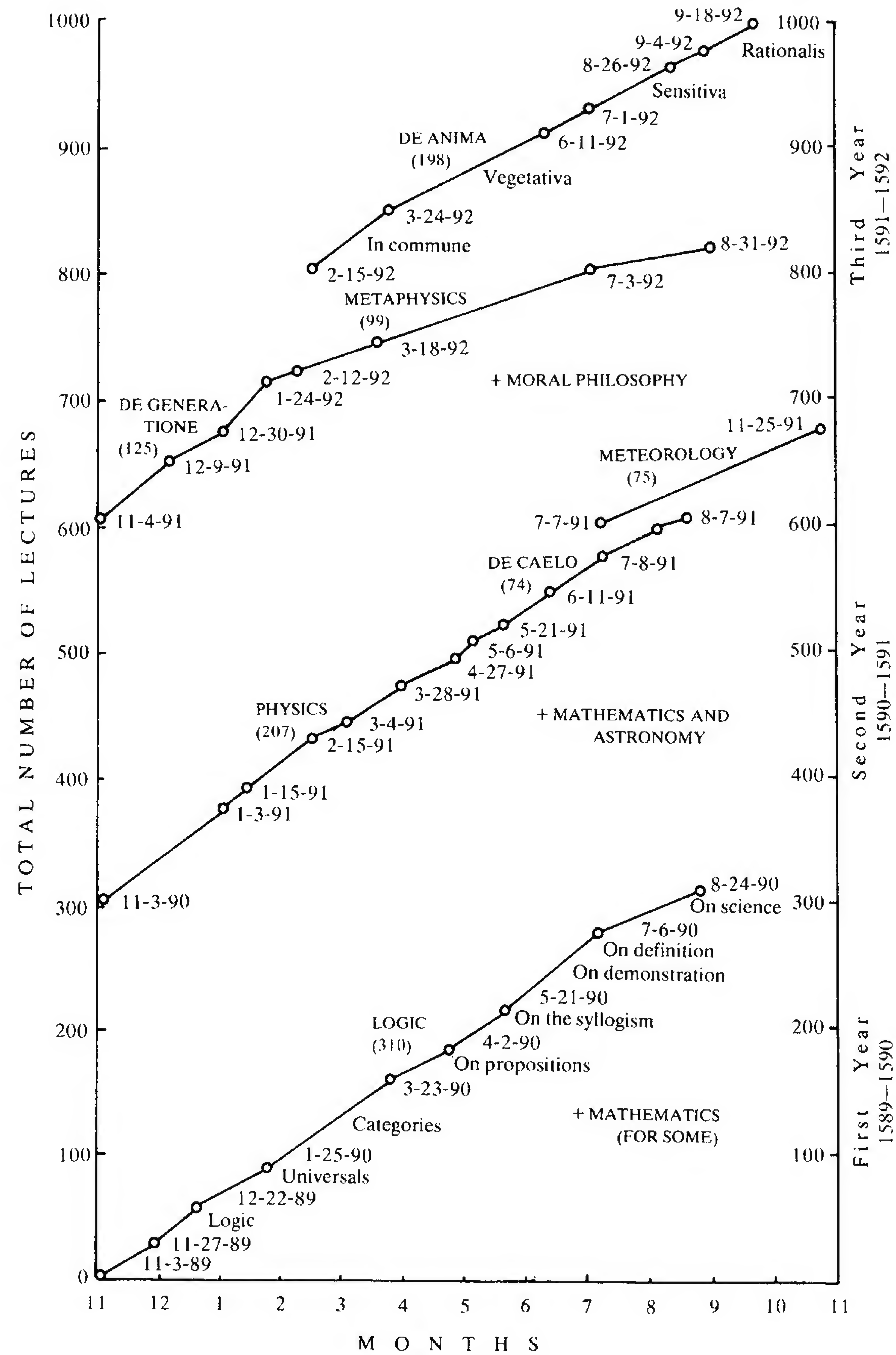

Fig. 1. LUDOVICUS RUGERIUS, Florentinus, S.I. Collegio Romano, 1589—1592
Cod SB Bamberg, Misc. Class. 62

Drake is quite right in anchoring his dates for Galileo's pre-Paduan writings on the time of composition of MS 27. A fortunate series of circumstances permits near-certain identification of the exemplar on which it is based, which in turn can be dated precisely. Since I have just published an exhaustive analysis of the manuscript and its sources, what follows is but a brief summary of the documentation on which my reasoning and the conclusions deriving from it are based.[15] The main result to which I come is that MS 27 could not have been written by Galileo before August of 1588, and most probably was composed by him in late 1588 or early 1589.

Six sets of notes from the course on logic at the Collegio Romano between the years 1584 and 1592 are available to substantiate this finding. Four of these are straightforward records of course work, whose professors and dates of completion are known to be as follows: Ioannes Lorinus (1584), Mutius Vitelleschus (1589), Ludovicus Rugerius (1590), and Robertus Jones (1592). The remaining two are connected with the course completed by Paulus Vallius in 1588, which turns out to be Galileo's exemplar. Vallius's original lectures are no longer extant, but portions of them were plagiarized by Ludovico Carbone and published by him in 1597.[16] Vallius himself subsequently became aware of the plagiarism and reworked his entire logic course, which he published in 1622.[17] These two versions thus reflect in varying ways Vallius's commentary on the *Posterior Analytics*. The amazing thing is that counterparts for both of Galileo's treatises in MS 27 can be found, either in Carbone (whom we shall henceforth designate as Vallius-Carbone, in view of the plagiarism, and where often there is word-for-word agreement), or in Vallius's revision. The similarities are so striking that they effectively rule out any other way of accounting for Galileo's organization and precise wording of the materials he appropriated in MS 27.

To elaborate somewhat on this analysis, and incidentally to furnish details that may prove helpful for dating MS 46, we furnish in Table 1 an outline of the contents of MS 27. The titles of the two treatises are given in Latin, as are also the disputations and questions making them up, the latter abbreviated where necessary.[18] The treatises are concerned respectively with "foreknowledges and foreknowns", meaning by this what has to be previously known or understood before one can

TABLE 1
Galileo: MS 27

[IN POSTERIORUM ANALYTICORUM ARISTOTELIS LIBROS]		
[Tractatio de praecognitionibus et praecognitis]		
. . . *[Filios possibly missing]* . . .		
De praecognitionibus et praecognitis in particulari		
De praecognitionibus principiorum		
An in omnibus principiis praecognoscendum . . . an sit	[a]	*6 pars.*
An de primis principiis praecognoscendum . . . quid nominis	[b]	*5 pars.*
An principia sint actualiter . . . praecognoscenda	[c]	*4 pars.*
An principia . . . nulla ratione probari possint	[d]	*8 pars.*
De praecognitionibus subiecti		
Quid intelligit Aristoteles nomine esse quando dicit de subiecto debere praecognosci an sit	[e]	*18 pars.*
An scientia possit demonstrare de suo subiecto adaequato esse existentiae	[f]	*10 pars.*
An scientia possit demonstrare an sit subiecti partialis	[g]	*9 pars.*
An scientia possit ostendere quid rei sui subiecti et reddere propter quid existentiae illius	[h]	*6 pars.*
Quid intelligit Aristoteles per praecognitionem quid sit quando de subiecto dicit . . . quid est quod dicitur	[i]	*4 pars.*
De praecognitionibus passionis et conclusionis		
An de passione praecognoscendum sit quia est	[j]	*13 pars.*
An conclusio cognoscatur simul tempore et natura cum cognitione praemissarum	[k]	*12 pars.*
Tractatio de demonstratione		
[De natura et praestantia demonstrationis]		
De definitione demonstrationis	[l]	*11 pars.*
An demonstratio sit nobilissimum . . . instrumentorum . . .	[m]	*37 pars.*
De proprietatibus demonstrationis		
An demonstratio constet ex veris	[n]	*9 pars.*
An demonstratio debeat constare ex primis et prioribus	[o]	*11 pars.*
Quid intelligat Aristoteles per . . . immediatas . . .	[p]	*10 pars.*
An omnis demonstratio constet ex immediatis, et quomodo	[q]	*7 pars.*
An omnia principia immediata per se nota ingrediantur quamcumque demonstrationem	[r]	*13 pars.*
An demonstratio constet ex notioribus, et an cognitio praemissarum sit . . . perfectior cognitione conclusionis	[s]	*16 pars.*
An demonstratio debeat constare ex propositionibus necessariis et de omni, et quomodo	[t]	*6 pars.*
[Quotuplex sit modus dicendi per se . . .]	[u]	*10 pars.*
Quot sint regulae cognoscendarum propositionum quae in primo et secundo modo continentur . . .	[v]	*11 pars.*
Qui sint modi demonstrationi inservientes	[w]	*7 pars.*

Table 1 (continued)

Quid sit praedicatum universale, et quae propositiones sub illo continenantur	**[x]**	*7 pars.*
An perfecta demonstratio debeat constare ex propositionibus per se, universalibus, et propriis	**[y]**	*15 pars.*
De speciebus demonstrationis		
Quot sint species demonstrationis	**[z]**	*18 pars.*
In quo conveniant et differant demonstratio propter quid et quia, et de huius divisione	**[yy]**	*3 pars.*
An detur regressus demonstrativus	**[zz]**	*14 pars.*

undertake to demonstrate a conclusion, and with "demonstration" itself, meaning by this apodictic proof. Two other treatises were frequently regarded as pertaining to the subject matter of the *Posterior Analytics*, one a *Tractatio de definitione* and the other a *Tractatio de scientia*. Galileo promises the second of these to follow his treatise on demonstration, but if he did write it, and this is not certain, it has not been preserved among his early manuscripts.[19]

The questions now extant in the manuscript are 28 in number, 11 pertaining to the treatise on foreknowledge and 17 to the treatise on demonstration; a few questions may be missing from the first treatise, since the beginning folios of the codex could have been lost. For purposes of later reference a lower-case letter has been assigned to each question; this is printed in bold face along the right margin, with the number of paragraphs the question contains indicated in italics. (Since there are only 26 letters in the alphabet, the last two questions are designated [yy] and [zz] respectively.) All of the questions are relatively short, [m] being the longest with 37 paragraphs and [yy] the shortest with 3, with the average being about 11.

Were one to analyze correlations with various possible exemplars, the procedure would be to make comparisons of each of the paragraphs in MS 27 with corresponding materials in the available logic courses to see precisely how much of the content of each question has counterparts in each possible exemplar. Apart from the content and degree of correlation, the ordering of the questions could also prove important, since the ordering might vary with each reorganization of the subject matter. Fortunately such procedures have not proved necessary for

determining the source and dating of MS 27, but they are mentioned here for their later application in the analysis of MS 46.

Table 2 provides a general overview of the correlations of Galileo's questions in MS 27 with those in the logic courses mentioned above. There is no breakdown of the questions paragraph by paragraph, as there will be in our analysis of MS 46; rather an X alone is used to signify when a particular question in Galileo's manuscript duplicates material contained in one or other logic course. One should note that the X's in the column for Vallius-Carbone are shown in bold face type, to indicate that they show practically word-for-word correspondences with Galileo's questions [a] through [k], and also [m]. As it turns out, Carbone did not plagiarize the treatise on demonstration but only that on foreknowledge and foreknowns, which explains why most of his correlations are with the first treatise. Almost by accident, however, he did appropriate one question from Vallius's second treatise, that on demonstration, namely [m], and this is a clue that the exemplar he

TABLE 2
Correlations of MS 27 with logic courses at the Collegio Romano

Galileo MS 27	Lorinus (1584) (1620)	Vallius-Carbone (1588–97)	Vitelleschus (1589)	Rugerius (1590)	Jones (1592)	Vallius (1622)
		DE PRAECOGNITIONIBUS ET PRAECOGNITIS				
		De praecognitionibus principiorum				
[a]*	X	**[X]***		X	X	X
[b]	X	**[X]**		X		X
[c]		**[X]**			X	X
[d]	X	**[X]**				X
		De praecognitionibus subiecti				
[e]	X	**[X]**		X	X	X
[f]	X	**[X]**			X	X
[g]	X	**[X]**				X
[h]		**[X]**				X
[i]	X	**[X]**		X		X
		De praecognitionibus passionis et conclusionis				
[j]	X	**[X]**		X	X	X
[k]	X	**[X]**	X	X	X	X
11	*9*	*11*	*1*	*6*	*6*	*11*

Table 2 (continued)

			DE DEMONSTRATIONE			
			De natura et praestantia demonstrationis			
[l]	X		X	X	X	X
[m]		**[X]**				X
			De proprietatibus demonstrationis			
[n]	X		X		X	X
[o]	X			X	X	X
[p]	X		X	X	X	X
[q]	X		X			X
[r]					X	X
[s]	X		X	X		X
[t]	X		X	X	X	X
[u]	X		X	X	X	X
[v]	X			X		X
[w]	X		X	X		X
[x]	X		X			X
[y]				X		X
			De speciebus demonstrationis			
[z]	X			X	X	X
[yy]	X			X	X	X
[zz]	X			X	X	X
17	*15*	*1*	*9*	*12*	*10*	*17*

Total number of questions in agreement:

28	*24*	*(12)*	*(10)*	*18*	*16*	*28*

* Bold face type indicates word-for-word similarity; totals in parentheses are for only a portion of the course.

worked from originally contained both treatises.[20] Galileo, it seems obvious, worked from the same exemplar or from a copy thereof.

The last column on the right, that for Vallius's reworked version, is the only column that lists correspondences for all 28 of Galileo's questions. In some questions there is word-for-word agreement, but this is not generally the case, nor is it to be expected, since Vallius himself acknowledges that he has changed the wording to differentiate his definitive work from Carbone's plagiarized version. Yet, since the revision was not published until 1622 whereas the lectures are completed in 1588, the question arises whether the entire contents of the second treatise, and particularly the all-important question dealing with the demonstrative *regressus* [zz], was present in Vallius's original lecture notes.

An affirmative answer to this question is indicated from a study of the course offered by Lorinus in 1584. As already noted, Lorinus stands at the head of the teaching tradition in logic at the Collegio just as Menu, as we shall see later, stands at the head of the teaching tradition on natural philosophy. Lorinus offered the logic course from 1583 to 1586 and then, like Vallius, published it, but not until 1620. The important difference is that his lecture notes dated 1584 are still extant, along with his published volume, and these show that he did not alter his wording at all in the intervening years. Now, in Table 2, it can be seen that most of Galileo's questions in MS 27 have correspondences in Lorinus's lectures of 1584 (24 of 28). The conclusion seems inescapable that Vallius based his lectures of 1588 on those given by Lorinus in the years preceding. Particularly in the second treatise, that dealing with demonstration, there are such strong correlations between Lorinus's lectures and Vallius's final version (15 of 17) that there must have been commerce between them. Simply on the basis of interchanging the first two columns in Table 2 one should be able to see that Galileo's notes (and thus Vallius's lectures) represent an appropriation, with only slight development, of the materials already taught by Lorinus.

A few remarks are in order about the remaining columns of Table 2. Vitelleschus covered the least matter in his course, omitting the first treatise almost entirely and covering only about a half of the material in the second. Jones was somewhat more thorough in his treatment of the matter in the lectures of Lorinus and Vallius (16 of 28 questions), but even when he discussed a particular question he did so in much sparser detail than either of his predecessors. Rugerius emerges as the best conserver of the tradition, showing the highest number of questions (18 of 28), and covering each of these in significant detail.

Apart from coverage of individual questions, as already noted an alternative way of tracing lines of influence is the ordering of questions within a particular treatise, since professors frequently put their imprint on materials by reordering them in various ways. (Vallius, in fact, in his preface to the 1622 version registered his displeasure with Carbone for reordering the questions he had plagiarized.[21]) Table 3 analyzes the same courses as presented in Table 2, only this time listing the questions not in the order found in MS 27 but in that in which the corresponding questions are found in the individual author. Many interesting observations could be made on the changes from one author

TABLE 3
The order of questions in logic courses at the Collegio, 1584—1592

Author	*De praecognitionibus*	*De demonstratione*	*Total*
LORINUS 1584	efgi j adb	l npqrksotuvwx zyyzz	24
VALLIUS-CARBONE 1588 1597	**ibacd j efgh k**	**m**	(12)
GALILEO MS 27	**abcd efghi jk**	**lm** nopqrstuvwxy zyyzz	28
VITELLESCHUS 1589		l npqkstuwx	(10)
RUGERIUS 1590	ab ei j	l nopkstuvwy zyyzz	18
JONES 1592	efja ck	l noprtu zyyzz	16
VALLIUS 1622	i bacdk j efgh	l npqrostuvwxy zyyzz m	28

Questions designated by letters in bold face type contain paragraphs with passages that show word-for-word similarity; totals enclosed in parentheses are those for portions of courses or of courses that are incomplete.

to another, but for purposes here it may suffice to note that Rugerius again emerges as the best indicator of the order found in Galileo's exemplar. One can see why Vallius was displeased with Carbone's work — even though he himself changed his ordering somewhat in his 1622 version — by comparing Vallius-Carbone's ordering with that of Galileo. Of all the logic teachers, even though he repeated only 18 of the 28 questions at issue, Rugerius best followed Vallius's ordering in his lectures of 1588. This fact has bearing on our analysis of Galileo's MS 46, as we are about to see.

Before leaving MS 27 it might prove interesting to address the following question: Supposing that Carbone's plagiarism had never been detected, and thus that the key piece of evidence for our dating of MS 27 were missing, how would one assign a date to Galileo's composition? The answer would have to be framed on the basis of the information contained in Tables 2 and 3, with the respective columns and rows for Vallius-Carbone and Vallius blocked out. *Prima facie*, on the basis of Table 2 one would have to favor Lorinus as the likely

source, and date Galileo's writing of MS 27 in 1584 or shortly thereafter. Rugerius would be the next best candidate, and his selection would move the likely date back to 1590. Table 3, on the other hand, might give ground for pause, since it shows that Rugerius best duplicates the order of Galileo's composition, and thus there could be reason, admittedly slight, to favor him. In either event the result would not be conclusive, and the best one might do is locate the composition somewhere between 1584 and 1590.

MS 46: THE PHYSICAL QUESTIONS

MS 46 is similar in many respects to MS 27, although it is much longer, being composed of 110 folios as opposed to the latter's 31. It too is an autograph, and Galileo's Latinity in it is much improved throughout, suggesting a later composition on the basis that he was more practiced in appropriating notes of this type. Folios are patently missing from MS 46, and this presents a problem to be discussed later. The extant materials, however, fall into three fairly distinct parts. Two of these, constituting the first 100 folios, are made up of treatises similar to those in MS 27. The third part consists of jottings or memoranda on motion that are obviously related to the *De motu* materials contained in MS 71, and whose discussion, on that account, is best postponed to our consideration of that manuscript.

The two parts or sets of treatises that take up the first hundred folios of MS 46 pertain to portions of a course in natural philosophy that deal with Aristotle's *De caelo* and *De generatione* respectively. These two works are in essential continuity within the Aristotelian corpus, and both discuss the elements, though from different points of view. Normally they would be treated as a unit in Renaissance instruction, following the *Physics* and preceding the *Meteorology*, but they could be separated at the Collegio; an example of such separation has already been seen in Figure 1. This perhaps can serve to explain why Galileo's treatises relating to the *De caelo* were written on paper different from those relating to the *De generatione* and probably at different times.

A detailed listing of the questions still extant in the first two parts of MS 46 is shown in Table 4. There are only 25 of these questions, and upper case letters have been assigned to each[22]; the numbers of paragraphs they contain are shown on the right. Generally the questions are much longer than those in MS 27; one alone, [K], has almost as

TABLE 4
Galileo: MS 46

[IN LIBROS ARISTOTELIS DE CAELO] *Folios 4^r–54^v*		
[Proemium]		
Quid sit id de quo disputat Aristoteles in his libris . . .	[A]	*21 pars.*
De ordine, connexione et inscriptione horum librorum	[B]	*9 pars.*
Tractatio de mundo		
De opinionibus veterum philosophorum de mundo	[C]	*9 pars.*
Quid sentiendum sit de origine mundi secundum veritatem	[D]	*8 pars.*
De unitate mundi et perfectione	[E]	*23 pars.*
An mundus potuerit esse ab aeterno	[F]	*27 pars.*
Tractatio de caelo		
An unum tantum sit caelum	[G]	*34 pars.*
De ordine orbium caelestium	[H]	*36 pars.*
An caeli sint unum ex corporibus simplicibus vel ex simplicibus compositi	[I]	*47 pars.*
An caelum sit incorruptibile	[J]	*36 pars.*
An caelum sit compositum ex materia et forma	[K]	*183 pars.*
An caelum sit animatum	[L]	*41 pars.*
. . . *[Folios probably missing]* . . .		
[IN LIBROS ARISTOTELIS DE GENERATIONE] *Folios 57^r–100^v*		
[Tractatus de alteratione]		
. . . *[Folios missing]* . . .		
[De alteratione]	[M]	*2 pars.*
De intensione et remissione	[N]	*32 pars.*
De partibus sive gradibus qualitatis	[O]	*9 pars.*
Tractatus de elementis		
De elementis in universum	[P]	*15 pars.*
De quidditate et substantia elementorum		
De definitionibus elementi	[Q]	*17 pars.*
De causa materiali, efficiente, et finali elementorum	[R]	*6 pars.*
Quae sint formae elementorum	[S]	*17 pars.*
An formae elementorum intendantur et remittantur	[T]	*21 pars.*
. . . *[Material missing between folios 74 and 75]* . . .		
[De numero et quantitate elementorum]	[U]	*80 pars.*
De primis qualitatibus		
De numero primarum qualitatum	[V]	*10 pars.*
An omnes hae quatuor qualitates sint positivae an potius aliquae sint privativae	[W]	*17 pars.*
An omnes quatuor qualitates sint activae	[X]	*21 pars.*
Quomodo se habeant primae qualitates in activitate et resistentia	[Y]	*25 pars.*
. . . *[Folios missing]* . . .		
[MEMORANDA DE MOTU] *Folios 102^r–110^v*		

many paragraphs as the larger of the two treatises in MS 27. Apart from a brief introduction, there are only four treatises in MS 46, the last of which contains two parts. As already noted the first two treatises, on the universe and on the heavens respectively, treat matters from *De caelo*, and the remaining two, on alteration and on the elements respectively, treat matters from *De generatione*. Folios are definitely missing from the beginning of the treatise on alteration and from the end of the treatise on the elements, and yet more folios are probably missing from the end of the treatise on the heavens. Some idea of the missing matter can be gained from the following survey of the sources on which Galileo's questions in this manuscript are likely based.

Three complete courses from the Collegio are available for purposes of comparison, and three other partial treatments supplement these. The complete courses are those of Antonius Menu, who taught *De caelo* and *De generatione* in 1578; of Mutius Vitelleschus, who taught the same in 1590; and of Ludovicus Rugerius, who did likewise in 1591.[23] The partial treatments include those of Christopher Clavius, from whose *Sphaera* of 1581 or 1585 Galileo's questions [G] and [H] seem to have been appropriated almost word-for-word, and of Paulus Vallius, whose expositions of *De caelo* and *De generatione* have apparently been lost, but who appended a *Tractatus de elementis* to a course he taught on the *Meteorology* some time between 1586 and 1589; this treatise shows strong correlations with Galileo's *Tractatus de elementis*, i.e., with questions [P] through [Y]. (The remaining document is a recently uncovered manuscript of Ludovico Carbone, which offers mainly confirmatory evidence, to be considered later in this essay.)

A listing of the treatises and questions in Menu's lectures on *De caelo* and *De generatione* is shown in Table 5. Those with counterparts in MS 46 are indicated there and in subsequent tables in italics. As already observed, Menu's role in developing the course on natural philosophy at the Collegio was similar to Lorinus's in developing the course on logic, and he is important on that account. His notes have been examined carefully for signs of agreement with Galileo's composition, and the results shown on the right of the table. Of the 183 paragraphs in Galileo's question [K], for example, materials agreeing with 106 of these can be found in Menu's lectures. For some questions, e.g., [C] through [F], shown in bold-face type, a substantial number of paragraphs contain passages with word-for-word similarity. For still others, e.g., [A], [B], [G], [H], and [V], there are no counterparts or correspondences whatever. Yet the gaps in the listings on the right

TABLE 5
Menu: Ueberlingen 138 (1578)

	GALILEO *Paragraph Agreement*	
IN LIBROS ARISTOTELIS DE CAELO		
Tractatus de mundo		
De origine mundi . . . opiniones . . .	[C]	*9 of 9*
. . . secundum veritatem . . .	[D]	*7 of 8*
An sit demonstrabile mundum fuisse factum in tempore		
An potuerit mundum esse ab aeterno	[E]	*5 of 27*
De unitate mundi, an unus tantum sit mundus	[F]	*8 of 8*
De perfectione mundi	[E]	*15 of 15*
Tractatus [de caelo]		
De natura et essentia coeli		
An coelum sit elementum vel compositum ex elementis	[I]	*35 of 47*
An caelum sit compositum ex materia et forma	[K]	*106 of 183*
An caelum ex sua natura sit incorruptibile	[J]	*17 of 36*
An caelum sit animatum . . .	[L]	*22 of 41*
Quid sit forma assistens et qua ratione . . . intelligentia dicatur forma assistens		
De accidentibus coeli		
De quantitate et figura		
De raritate, densitate, duritie, perspicuitate, opacitate coeli		
An in coelo sit differentia positionum . . .		
De motu et de subiecto motus caeli		
An caelum movetur ab intelligentia vel a propria forma		
An motus circularis coeli sit naturalis		
An astra habeant lumen ex sua natura vel id recipiant a sole		
De actione coeli		
An coelum agat in haec inferiora		
An et quando aget coelum per lumen		
An coelum agat per influentias		
[IN LIBROS ARISTOTELIS DE GENERATIONE]		
[Tractatus de generatione]		
[De causis generationis]		
De subiecto generationis		
De forma quae per generationem acquiritur		
De efficienti generationis		
De quidditate generationis		
[Tractatus de alteratione]		
[Quid sit alteratio]		
Ad quas qualitates sit alteratio	[M]	*1 of 2*
An et quomodo alteratio sit motus continuus	[N]	*6 of 32*
Quomodo fiat intensio et remissio	[O]	*5 of 9*

Table 5 (continued)

[Tractatus de actione et passione]		
Quid sit actio et passio . . .		
An omnis actio fit per contactum et quomodo		
An idem possit agere in seipsum et qua ratione		
An sit admittenda actio reflexa eiusdem in seipsum		
An simile agere possit in simile et quomodo		
[Tractatus de elementis]		
De elementis in genere		
De nomine elementi et aliis	[P]	*12 of 15*
Quae scientia tractet de elementis . . .		
Propositio eorum quae in sequentibus . . . tractanda sunt		
De existentia elementorm		
De causa finali et efficiente elementorum	**[R]**	*4 of 4*
De materia elementorum	**[R]**	*2 of 2*
Quae sint formae substantiales elementorum	[S]	*13 of 17*
An formae elementorum intendantur et remittantur	[T]	*10 of 21*
De definitionibus elementorum	[Q]	*12 of 17*
[De numero et distinctione elementorum]		
[De quantitate, transmutatione, et aliis accidentibus elementorum]		
An elementa immediate ad invicem transmutari possint		
Quomodo in elementis symbolis sit faciliter transitus . . .		
De maximo et minimo elementorum	[U]	*27 of 74*
De raritate et densitate . . .		
De loco elementorum		
De figura elementorum		
De proportione elementorum		
De numero et puritate elementorum		
De qualitatibus alterativis		
De numero qualitatum	[U]	*6 of 10*
An quatuor qualitates primae sint reales et positivae	[W]	*10 of 17*
Cur caliditas et frigiditas dicatur activae . . .	[X]	*6 of 21*
Quomodo se habent qualitates in resistendo	[Y]	*4 of 25*
Definitiones humidi, calidi, etc.		
An ambo qualitates sint in summo gradu . . .		
Qualitates symobolorum mediorum elementorum		
De qualitatibus motivis		
Quid sit gravitas et levitas		
Unde proveniant gravitas et levitas		
Qualitates motivae mediorum elementorum et simplices . . .		
A quo fiat motus elementorum		
De motu violento proiectorum gravium et levium		

point to large portions of Menu's material that Galileo might have incorporated into notes that have since been lost — the missing folios referred to above. These would include substantial amounts relating to the accidents of the heavens and the actions of the heavens on the sublunary world, entire treatises on generation and on action and passion, and a goodly part of the treatise on the elements, particularly the part relating to their motive qualities, with its lengthy discussion of *gravitas* and *levitas* — extremely important for anyone interested, as was Galileo, in tracts *De motu gravium et levium*.

Following in chronological order, the next important possible source for Galileo's MS 46 (apart from Clavius's *Sphaera*) is Vallius's *De elementis*. It is difficult to date this treatise, which is preserved at the end of a codex containing Vallius's undated lectures on the *Meteorology*.[24] Following the order of the Aristotelian corpus, the *Meteorology* should be taught after the *De generatione*, but some adjustments were made in that order at the Collegio because of the large amount of natural philosophy that had to be covered in the *Cursus philosophicus*. In 1591, as can be seen in Figure 1, Rugerius squeezed the *Meteorology* in between the *De caelo* and the *De generatione*, lecturing simultaneously on it and the *De caelo* during July of that year. Earlier, in 1578, Menu followed a similar procedure, teaching the *Meteorology* simultaneously with the first portion of the *De generatione*. A *reportatio* of lectures given by Vallius in 1585 under the title *De mixtis inanimatis imperfectis et perfectis*, the first part of which would correspond to the *Meteorology*, is known to have existed at one time but I have not been able to locate it.[25] Again, in a promised revision of his entire course on natural philosophy described in 1622, Vallius proposed to treat *De elementis* in conjunction with *De generatione*.[26] Piecing these pieces of information together, since Vallius most likely taught *De generatione* at the beginning of the third year of the philosophy cycle (as did Rugerius in 1591), and did so in 1585, 1586, and 1589, one could assign any one of these dates to the treatise on the elements that has been preserved.

Its dating aside, the contents of the treatise are shown in Table 6, arranged in the same format as Table 5. As can be seen from the "Paragraph Agreement" on the right, all of Galileo's questions show substantial agreement with the first two sections of Vallius's *Tractatus*, many of them containing passages with word-for-word agreement.

TABLE 6
Vallius: APUG/FC 1710 (1585—1589?)

	GALILEO	
Tractatus de elementis	*Paragraph*	
De elementis in genere	*Agreement*	
De essentia elementorum		
An dantur elementa in rerum natura	[P]	*14 of 15*
De causa efficiente elementorum	[R]	*6 of 6*
De forma et materia elementorum	[S]	*15 of 17*
An formae substantiales elementorum intendantur . . .	[T]	*14 of 21*
De definitionibus elementi in genere	[Q]	*13 of 17*
De numero et distinctione elementorum	[U]	*3 of 6*
De qualitatibus activis elementorum		
An sint tantum quatuor qualitates primae	[V]	*6 of 10*
An omnes primae qualitates sint activae . . .	[X]	*10 of 21*
et positivae . . .	[W]	*14 of 17*
Quomodo se habeant hae qualitates in actione resistentiae	[Y]	*9 of 25*
De definitionibus harum qualitatum		
An qualitates simbole sint eiusdem speciei . . .		
An in elementis utraque qualitas sit in summo		
An praeter qualitates actuales dentur . . . virtuales		
De transmutatione elementorum		
An elementa sint adinvicem transmutabilia		
An omnia elementa sint immediate ad invicem transmutabilia		
An sit facilior transitus in elementis simbolis quam in dissimbolis		
An et quomodo est duobus elementis dissimbolis fiat tertium ab illis distinctum		
De quantitate elementorum		
An dantur maximum et minimum in elementis	[U]	*41 of 74*
An in elementis detur proportio quoad quantitatem		
Quid et quotuplex sit raritas et densitas . . .		
An in rarefactione acquiratur quantitas de novo . . .		
De figura et puritate elementorum in suis spheris		
De qualitatibus motivis		
Quid sit gravitas et levitas		
Unde proveniat levitas et gravitas		
An qualitates motivae mediorum elementorum sint simplices et different specie ab extremis		
De loco elementorum		
An elementa moveantur a seipsis an vero a generante . . .		
A quo moveantur proiecta		

Table 6 (continued)

De elementis in particulari
An detur ignis elementaris
De proprietatibus ignis
An aer sit ex natura sua calidus
An aqua sit maior et frigidior terra
An terra sit gravissima et siccissima
An aer, aqua, et terra gravitent in suis sphaeris
Tractatus de mixtis perfectis
. . .
[Tractatus de mixtis imperfectis]

Apart from Galileo's question [U], which is separated from his previous questions by blank pages anyway,[27] none of Vallius's last four sections has a counterpart in MS 46. These sections, like those in Menu, contain important treatments of the transmutation of the elements, their quantity, their motive qualities, projectile motion, and the answers to such questions as "Does air have weight in air?", much of which occupied Galileo's attention in MS 71. They too, therefore, are suggestive of the type of material appropriated by Galileo on the missing folios of MS 46, once present there but since lost. Moreover, of all the notes relating to *De caelo* and *De generatione*, this treatise by Vallius shows the best agreement with Galileo's MS 46, in a few instances as good as the agreement between Vallius's notes on logic and Galileo's MS 27. While far from apodictic, these are persuasive lines of argument that point to Vallius as the most likely source of both Pisan manuscripts.

Vitelleschus and Rugerius, in that order, followed Vallius in covering the materials of *De caelo* and *De generatione* in 1590 and 1591 respectively. Their value, as will be seen, is similar to that noted in Tables 2 and 3 above, where they basically support Vallius's candidacy by preserving vestiges of his notes, notes that presumably were once available to Galileo. Table 7 gives an outline of the course offered by Vitelleschus, again in the same format as Tables 5 and 6. Note that the contents include an entire course made up of eight treatises in all; they are therefore different from Vallius's material in Table 6, which shows the content of but a single treatise. Also noteworthy is the presence of *two* treatises on the elements in Vitelleschus's course: the first cor-

TABLE 7
Vitelleschus: APUG/FC 392 (1589—1590)

	GALILEO *Paragraph Agreement*	
IN LIBROS DE CAELO		
De obiecto horum librorum	[A]	*13 of 21*
Tractatio de mundo		
Quid de origine mundi senserit Aristoteles	[C]	*5 of 9*
An lumine naturali cognosci possit mundum fuisse a Deo creatum in tempore	[D]	*3 of 8*
Solutio eorum quae pro mundi aeternitate . . . afferantur		
An mundus potuerit esse ab aeterno	[F]	*14 of 27*
De unitate et perfectione mundi	[E]	*13 of 23*
Tractatio de caelo		
An caelum sit aliquod elementum vel ex [eis] compositum	[I]	*29 of 47*
An caelum sit compositum ex materia et forma	[K]	*34 of 184*
An caelum sit animatum	[L]	*32 of 41*
De distinctione caelorum et astrorum inter se		
An in caelo sint differenciae positionum, et quomodo		
An caelum sit corruptibile	[J]	*25 of 36*
De quantitate et aliis caeli accidentibus . . .		
De lumine caeli in particulari		
De motu caeli et primum de intelligentia movente		
An motus caeli sit naturalis et quomodo		
An et quomodo caelum agat per motum		
An caelum alio modo agat in haec inferiora		
An cessante motu caeli cessarent actiones . . . corporatorum		
Tractatio de elementis		
An sit gravitas et levitas et quomodo		
Unde oriantur gravitas et levitas . . .		
De qualitatibus motivis mediorum elementorum		
An elementa gravitent et levitent in propriis sphaeris		
A quo moveantur elementa		
An et cur gravia et levia moveantur velocius in fine . . .		
IN LIBROS DE GENERATIONE		
De obiecto horum librorum		
An generatio unius sit corruptio alterius et contra		
[Tractatio de alteratione]		
Quid sit alteratio	[M]	*1 of 2*
An alteratio sit motus continuus	[N]	*25 of 32*
Quomodo qualitates intendantur . . . in alteratione	[O]	*7 of 9*
An hi gradus quibus intenditur qualitas sit homogenii . . .		

Table 7 (continued)

Quid sit id quod dicitur proprie generatio		
An accidentia inhereant materiae primae an composito		
An possit ullum accidens . . . in corrupto idem remanere in genito		
An agentia particularia vere generant tanquam . . . principales . . .		
An substantia agat immediate		
An et quomodo accidens producat substantiam . . .		
Tractatio de augmentatione		
Quid sit formaliter augmentatio		
An augmentatio sit motus continuus		
An quantitas distinguatur realiter a re quanta		
De quantitate interminata materiae		
De maximo et minimo . . .	[U]	*39 of 74*
Quid sit raritas et densitas		
An tanta vel tanta extensio sit de essentia quantitatis		
An in rarefactione acquiratur nova quantitas . . .		
Tractatio de actione et passione		
An ad actionem requiratur contactus agentis et patientis		
An aliquid possit agere in seipsum		
De reductione elementorum ad primum statum		
Quid sit antiperistasis et quomodo fiat		
An simile agat in simile		
An inter res naturales . . . detur reactio	[Y]	*4 of 9*
Quomodo se habeat in reactione pars repassa		
Tractatio de mixtione		
An elementa maneant in mixto virtute vel actu		
An formae elementorum in mixto maneant integrae an refractae		
An formae elementorum refractae maneant in mixto actu . . .		
De causa materiali mixtionis		
De causa efficiente mixtionis		
De causa formali mixtionis et quomodo fit . . .		
Tractatio de elementis ut sunt materia mixtionis		
De numero elementorum	[U]	*3 of 6*
Quid sit elementum	[PQ]	*5 of 32*
Quae sint formae elementorum	**[S]**	*12 of 17*
An quatuor primae qualitates sint omnes reales	[W]	*15 of 17*
An omnes qualitates praedictae sint [aeque] primae	[V]	*4 of 10*
An omnes quatuor primae qualitates sint activae . . .	**[X]**	*15 of 21*
An hae qualitates sint magis activae quam resistivae	**[Y]**	*10 of 16*
An bene definiantur ab Aristotele . . . qualitates primae . . .		
An in elementis sint qualitates primae summe intensae . . .		
An quodcumque elementum possit immediate transmutari . . .		
An facilior et velocior sit transitus inter . . . simbola . . .		
An ex duobus elementis dissimbolis possit tertium generari . . .		

responds to Aristotle's text in Books 3 and 4 of *De caelo*, where he discusses the elements as they are parts of the universe and move locally, and the second to Aristotle's text in Book 2 of *De generatione*, where he discusses the elements as they act on each other and become parts of compounds. Correspondences for 20 of Galileo's 25 questions are shown to the right of the table. Some questions, shown in bold-face type, contain passages with almost word-for-word agreement with Galileo's text, notably [F], [L], and [S]. The spread is quite good over all four of Galileo's treatises, but significantly none of Galileo's *De elementis* corresponds to Vitelleschus's first treatise in the *De caelo*, all of it having counterparts in his second. In this feature Vitelleschus strengthens the impression one gets from reading Menu and Vallius, namely, that the folios missing from MS 46 were probably concerned with the elements as treated by Aristotle in the *De caelo*, that is, as they are heavy and light and so move locally to different parts of the universe.

Rugerius's course on *De caelo* and *De generatione* is similarly shown in Table 8. His thoroughness here matches that already seen in his

TABLE 8

Rugerius: Bamberg 62 (1590—1591)

IN LIBROS DE CAELO ET MUNDO	GALILEO	*Paragraph Agreement*
Disputatio de universo seu de corpore simplici in universum		
Quid et quomodo agatur ab Aristotele in libris de caelo		
Quid sit subiectum horum librorum	[A]	*18 of 21*
Quaenam sit horum librorum . . . connexio cum caeteris	[B]	*5 of 9*
Quaenam sit horum librorum inscriptio		
Quaenam sit horum librorum partitio . . .		
An mundus et motus sit aeternus	[CD]	*12 of 17*
Quonam modo mundus et motus esse coeperit		
An motus et mundus potuerit esse ab aeterno	[F]	*21 of 27*
Disputatio de corpore caelesti		
Tractatus de natura caeli		
An caelum sit naturae elementalis		
An caelum sit unum ex quatuor elementis, [vel] . . . compositum ex elementis	[I]	*18 of 47*
An Aristoteles . . . demonstret caelum esse . . . elementum		
An caelum sit compositum ex materia et forma	[K]	*28 of 183*
An caelum sit animatum	[L]	*24 of 41*

Table 8 (continued)

Tractatus de partibus caeli		
Quot et quales sint orbes		
An et quales sint poli in caelo		
Qualis et unde stellarum varietas existat		
Tractatus de motibus et aliis accidentibus . . . caeli		
An caelum sit ingenerabile et incorruptibile	[J]	*18 of 36*
An caelum moveatur ab intelligentia, et quomodo		
Quae et qualia sint reliqua accidentia caeli		
Tractatus de actione caeli		
An et quomodo caeli [agat] in inferiora per motum		
An et quomodo caeli agat per lumen . . .		
An caelum agat per . . . qualitates occultas, nempe influentias		
Disputatio de gravi et levi		
Tractatus de qualitatibus motivis gravium et levium		
Quid sit gravitas et levitas		
An gravitas et levitas sint qualitates primae		
An omnes qualitates motivae inter se specie differant		
An elementa gravitent et levitent in propriis locis		
Tractatus de motu locali gravium et levium		
A quo naturaliter moveantur gravia et levia		
A quo moveantur proiecta		
Qualis sit motus elementorum in orbem		
Quomodo se habeant motus gravium et levium ratione velocitatis et tarditatis . . .		
IN LIBROS DE GENERATIONE		
Disputatio de generatione in communi . . .		
Tractatus de generatione et corruptione		
Quid intelligitur nomine generationis et corruptionis . . .		
Quale sit subiectum generationis		
Qualis sit terminus ad quem generationis		
Quae et qualis sit causa efficiens generationis		
Tractatus de alteratione		
Quid sit alteratio	[M]	*1 of 2*
Quomodo fiat, quid sit intensio et remissio qualitatum	[NO]	*23 of 35*
An et quomodo alteratio sit motus continuus	[N]	*6 of 6*
Tractatio de augmentatione		
Quid sit augmentatio		
Quae sint conditiones augmentationis		
Disputatio de generatione et corruptione mixtorum		
Tractatus de . . . tactu, actione, et passione		
. . .		
Tractatus de mixtione et putrefactione		
. . .		

Table 8 (continued)

Disputatio de elementis prout ad mixtionem ordinantur		
Tractatio de substantia elementorum		
Quid significet nomen elementi	**[P]**	*11 of 15*
Quomodo definiatur elementum	[Q]	*12 of 17*
Quae sint formae substantiales elementorum	[S]	*4 of 17*
An formae elementorum suscipiant magis et minus	[T]	*11 of 21*
Tractatus de numero et quantitate elementorum		
Quot sint elementa	[U]	*10 of 15*
An et qualis sit ignis elementaris		
An detur elementa pura		
Quae et qualis sit quantitas continua elementorum	[U]	*13 of 65*
Tractatus de qualitatibus activis et passivis elementorum		
An omnes quatuor qualitates alterative sint . . . reales	**[W]**	*15 of 17*
An quatuor qualitates omnes et solae sint primae	[V]	*7 of 10*
Quae et quales sint definitiones harum qualitatum . . .	**[X]**	*8 of 21*
An omnes hae qualitates sint activae et passivae . . .	[Y]	*7 of 25*
Undenam et quare elementa habeant has quatuor qualitates		
An ambae qualitates unius elementi sint aequales		
An qualitates simbolae elementorum sint eiusdem speciei		
. . .		
An in elementi sint aliae qualitates praeter primas		
Tractatus de mutua transmutatione elementorum		
An elementa sint transmutabilia		
An quodlibet elementum in quodlibet aliud transmutari possit		
Quomodo fiat mutua elementorum transmutatio		
Disputatio de forma et efficiente mixtionis		
Tractatus de causa formali mixtionis		
. . .		
Tractatus de causa efficiente mixtionis		
. . .		

course on logic, for he has 17 treatises where Galileo preserves only 4; in them the coverage is likewise good, for there are counterparts for 21 of Galileo's 25 questions. Here too there are questions containing passages with word-for-word agreement with Galileo's text and so shown in bold-face type, namely, [P], [W], and [X]. But again, neither of the two treatises in Rugerius's *Disputatio de gravi et levi*, those concerned with motive qualities and the local motion of heavy and light bodies, is to be found in Galileo's notes. This reinforces the suspicion that such treatises were probably available to Galileo, even appropriated by him, but on the missing folios and so no longer extant.

With this presentation of likely sources of MS 46 we are in a position to sum up our analysis of that manuscript along lines similar to that already provided in Tables 2 and 3 for MS 27. Table 9 is the counterpart of Table 2 but differs from it in the important respect that, while Table 2 shows only generic agreement by means of an X, Table 9 records the fine structure of the agreement in two ways. The first is by recording the total number of paragraphs in the author's questions that correspond to Galileo's, taken from Tables 5 through 8, and the second by indicating, in parentheses, how many of these paragraphs contain passages that register word-for-word agreement.

One striking feature of Table 9 is the role played by Menu in setting up the basic course in 1578, quite analogous to that played by Lorinus in his pioneering logic course of 1584. A number of Menu's questions in the portion of the course dealing with the *De caelo* show strong correlations with Galileo's questions [C] through [L], suggesting either that Menu's notes were directly available to him, possibly in a version revised after 1578 (for Menu did teach the *De caelo* again in 1580—

TABLE 9
Correlations of MS 46 with physics courses at the Collegio Romano

Galileo MS 46		Menu 1578	Clavius 1581	Vallius 1585–1589	Vitelleschi 1590	Rugerius 1591	Carbone 1584
				IN LIBROS DE CAELO			
				Proemium			
[A]	(21)				13	18	X
[B]	(9)					5	X
				De mundo			
[C]*	**(9)**	**9 (8)**			5	8	
[D]	**(8)**	**8 (7)**			3	4	X
[E]	**(23)**	**23 (11)**			13		X
[F]	**(27)**	5			**14 (4)**	21	
				De caelo			
[G]	**(34)**		**34 (34)**				X
[H]	**(36)**		**36 (36)**				X
[I]	**(47)**	**35 (2)**			29	18	X
[J]	(36)	17			25	18	X
[K]	(183)	106			34	28	X
[L]	**(41)**	22			**32 (3)**	24	X
	474	*225 (28)*	*70 (70)*		*168 (7)*	*144*	

Table 9 (continued)

				IN LIBROS DE GENERATIONE			
				De alteratione			
[M]	(2)	1			1	1	X
[N]	(32)	2			25	23	X
[O]	(9)	5			7	6	
				De elementis			
[P]	**(15)**	**12 (1)**		14	2	**11 (4)**	X
[Q]	**(17)**	12		**13 (6)**	3	12	X
[R]	**(6)**	**6 (4)**		6			
[S]	**(17)**	13		**15 (2)**	**12 (4)**	4	X
[T]	**(21)**	**10 (1)**		**14 (2)**		11	X
[U]	**(80)**	**33 (1)**		**44 (8)**	42	23	X
				De primis qualitatibus			
[V]	(10)			6	4	7	X
[W]	**(17)**	**10 (2)**		**14 (7)**	15	**15 (2)**	X
[X]	**(21)**	6		10	**15 (2)**	**8 (4)**	X
[Y]	**(25)**	4		9	**14 (1)**	7	
	272	*114 (9)*		*145 (25)*	*140 (7)*	*128 (10)*	
	746	339 (37)	70 (70)	145 (25)	308 (14)	272 (10)	
Number of questions in agreement:							
	25	20	2	10	20	21	20

* Bold face and number of paragraphs in parentheses indicate word-for-word similarity

1581), or else that Menu's notes were appropriated by a successor and so passed on to him indirectly. In the latter case the successor could have been Mutius De Angelis, who taught the course in natural philosophy continuously from 1584 to 1587, or Iacobus Caribdus, who taught it in 1587—1588, but neither of whose notes are available to me.[28] More probably, however, it was Vallius, whose lectures of 1589 on *De caelo* and *De generatione* were then passed on to Vitelleschus and Rugerius, as was the case with his notes of the previous year on the *Posterior Analytics.*

The latter possibility is suggested by the fairly strong agreement in the next two versions of the course on *De caelo* and *De generatione*, taught by Vitelleschus and Rugerius in 1590 and 1591 respectively. In Vallius's case, as can be seen, the correlations are only for the treatise on the elements added to his lectures on the *Meteorology*, correspond-

ing to the correlations for the treatise shown under Vallius-Carbone in Table 2, but not nearly so good as the latter. I suspect, therefore, that the *Tractatus de elementis* for which correlations have been made was *not* Galileo's exemplar for MS 46, but rather the complete set of notes for the lectures Vallius gave on *De caelo* and *De generatione* in 1589. As we know from his revision of his logical notes, Vallius was a prodigious worker and continually revised his teaching materials. If he saw to it that Galileo received his course on the *Posterior Analytics* of 1588 for the note-taking that shows up in MS 27, it seems reasonable to suppose that he would want Galileo to have his most recent course on *De caelo* and *De generatione* for the note-taking that similarly shows up in MS 46.

This line of thought would seem to be confirmed by the data presented in Table 10, analogous to those previously given in Table 3. Note here that there are no counterparts for Galileo's question [A] prior to 1590 or for his question [B] prior to 1591, and that question [V] has no precedent prior to Vallius's *De elementis*. Also, despite minor variations in the ordering of the questions, Vitelleschus and Rugerius best preserve Galileo's ordering in MS 46. The early anticipations in Menu's notes of 1578 need not be a sign of Galileo's copying from them directly, any more than the anticipations in Lorinus's logic notes of 1584 were such a sign of Galileo's direct use of Lorinus for MS 27. An indirect influence of these earlier notes would be sufficient to explain all of the correlations. Similarly, the preservation of more extensive correlations in Vitelleschus and Rugerius need not be a sign of Galileo's having copied from these later authors.[29] Thus, arguing *a pari* from the materials presented in Tables 2 and 3, the materials presented in Tables 9 and 10 point to Vallius as the Collegio professor who was likely behind the composition of MS 46, just as we know him to have been the professor who was actually behind the composition of MS 27.

DATING THE *DE CAELO* PORTION OF MS 46

With this we are in a position to return to Drake's dating of MS 46 on the basis of the watermarks on the paper that make up the three parts of the manuscript. As already intimated, he puts the composition of these parts at widely separated intervals, maintaining that the treatises relating to *De caelo* were written at Pisa in 1584,[30] that those relating

TABLE 10
The order of questions in physics courses at the Collegio, 1578—1594

Author	*De caelo*			*De generatione*					*Total*
MENU 1578		**CDFE**	IKJL	MNO		**PRSTQU**		WXY	20
CLAVIUS 1581			**GH**						(2)
VALLIUS 1589				*[De elementis]* **PRSTQU**		VWXYU			(10)
GALILEO MS 46	AB	**CDEF**	**GHIJKL**	MNO		**PQRSTU**		**VWXY**	25
VITELLESCHUS 1590	A	CDFE	**IKLJ**	MNO	U Y	**UPQS**		WV**XY**	(20)
RUGERIUS 1591	AB	CD F	IKLJ	MNO		**PQ** STU		**WVXY**	21
CARBONE 1594	AB	DE	IKLJGH			PQSU	MN	WTVX	20

Questions designated by letters in bold face type contain paragraphs with passages that show word-for-word similarity; totals enclosed in parentheses are those for portions of courses or for courses that are incomplete.

to *De generatione* were written at Florence in 1588, and that the memoranda on motion were composed at Florence and Pisa at various times between 1587 and 1590. According to Drake's dating, therefore, the physical questions of MS 46 were written *before* the logical questions of MS 27, and either before or contemporaneously with the sequence of memoranda relating to the *De motu* treatises.

The most aberrant of these dates is obviously that assigned to the treatises relating to *De caelo*, for which Drake still finds convincing the argument of Antonio Favaro, who in the National Edition labeled these treatises and those relating to *De generatione* "youthful writings", or *Juvenilia*, and placed them in 1584, while Galileo was still a student at the University of Pisa. Favaro did so on the basis of internal evidence, which he erroneously constructed, in my view, from a biblical chronology recorded by Galileo in the last paragraph of question [D]. The details of that chronology are discussed elsewhere;[31] for purposes here the important point to note is that Galileo there gives the interval between the destruction of Jerusalem and "the present time" as 1510 years. Since Jerusalem was destroyed in A.D. 70, the only possible dating a scholar can derive from Galileo's reference to the present time is 70 plus 1510, or A.D. 1580.[32] In previous discussions of dating based on this statement in MS 46 I have not hesitated to discount 1580 as the actual time of Galileo's writing. Such an early dating is ruled out by Galileo's youth, since in that year he was only sixteen years of age and had not yet begun his studies at the University of Pisa. I have never denied, however, that the year 1580 may have been indicated in the exemplar on which the notes of MS 46 were based, and thus have argued either that Galileo gave up on his attempt to recalculate the interval to the actual time of his writing, and so simply copied the interval given in his source, or else made an error in calculating and wrote 1510 where he should have written 1520, which would have yielded 1590, a date corroborated by other substantial evidence.[33]

It is noteworthy that Drake's argument in favor of 1584 is not based on watermarks, since all one can deduce from watermark evidence is that the notes were written at Pisa, which I and others have consistently maintained. Drake holds that the writing of MS 46 was prompted by Galileo's desire to obtain a position teaching natural philosophy, since "his father had warned him not to expect support beyond the academic year 1584—1585",[34] and thus he felt impelled to provide for his future. Granted that such might have been his motivation, it seems unlikely

that Galileo would have had access to the materials found in MS 46 at that early date. During his student days at Pisa (1581—1585) the *De caelo* was taught at the University only once, in 1583, when the professors were Hieronimus Borrus, Franciscus Bonamicus, and Franciscus Verinus.[35] None of the writings of these men bears any resemblance to Galileo's note-taking. The questions contained in MS 46 are obviously of Jesuit provenance. How can Drake explain Galileo's contacts with the Jesuits at that early date? Why, moreover, should Roman Jesuits have been interested in furthering the academic future of a 20-year old youth in Pisa, one who was not their student and who gave no promise of advancing the causes in which they were interested?

Given the basic fact of provenance from the Roman Jesuits — so amply demonstrated in the tables analyzed above — the key to the dating of MS 46 must continue to be MS 27, for it alone can provide a satisfactory explanation of Galileo's contacts with the Collegio Romano and his motivation for appropriating the extensive series of notes contained in the two manuscripts. As I have argued elsewhere,[36] it was Galileo's interest in mathematics, not his interest in natural philosophy, that brought about the initial interchange. Some time after leaving the University of Pisa in 1585, Galileo composed his *Theoremata circa centrum gravitatis solidorum* and sought for it the approbation of eminent mathematicians, including Christopher Clavius. Late in 1587, while in Rome, Galileo visited Clavius at the Collegio and left with him theorems from the work. Clavius was impressed by them and wrote to Galileo on January 16, 1588 and again on March 5, 1588; he did point out, however, what he thought to be a *petitio principii*, and thus a flaw, in Galileo's logical methodology. It is possible that Clavius detected the *petitio* during Galileo's visit to him in 1587 and took the occasion to introduce Galileo to Vallius, who at the time was teaching the logic course at the Collegio, to explain what legitimately could be presupposed in a demonstration and what could not. Alternatively, he may have noted the *petitio* during a more careful reading of the theorems after Galileo's departure, and subsequently put the two in touch. In any event, by August of 1588 Vallius had finished the course, and shortly thereafter, as he himself revealed, made his notes available to his students.[37] Ludovico Carbone acquired one set, which he later plagiarized, and presumably Galileo gained access to another. It is significant that the expression *petitio principii* (or its variant Latin forms) occurs frequently in Vallius's lecture notes, giving credence to

this line of reasoning.[38] Allowing some time for Galileo to read through the notes, block out the sections that interested him, and then summarize them for himself, I arrive at early 1589 as the probable date for Galileo's writing MS 27, and his doing so in Pisa, which accounts for the Pisan watermarks.

Two important pieces of internal evidence connect MS 46 with MS 27, and show incidentally that MS 46 could not have been written before MS 27, as Drake maintains. The first is the terminology employed in MS 46, which presupposes a detailed knowledge of Aristotelian demonstrative logic and the requirements of scientific reasoning; the terminology would be unintelligible, and hence unusable, to one unacquainted with the *Posterior Analytics*. Yet, in MS 46, Galileo treats these difficult matters competently, making no notable errors in his appropriation of the materials contained in his exemplar. In two places, moreover, Galileo presupposes knowledge of conclusions he has already proved in MS 27.[39] The second is Galileo's competence in Latin composition, which, as already remarked, is poor in MS 27 and quite passable in MS 46. William Edwards and I have just completed an exhaustive study of misspellings and ungrammatical syntax in MS 27, and our results amply confirm the conclusion to which Favaro had come, namely, that the notes, while intelligible, are still the work of a neophyte.[40] No such judgment need be made of the Latinity in MS 46 or MS 71. The process here, it must be stressed, is irreversible: one improves with practice in Latin composition, particularly when long time intervals do not separate successive writings, and thus the better the Latinity the later the time of writing.

Once we have established that MS 46 was written *after* MS 27, the next question is when and for what purpose the composition of this longer manuscript was undertaken. If MS 27 occupied Galileo's time in the early part of 1589, it seems reasonable to expect that he would not have started on MS 46 until later that year. In the summer of 1589 Father Philippus Fantonius, the mathematician who had been teaching the *Sphaera* and the *Theorica planetarum* at the University of Pisa, vacated his position and Galileo was appointed to succeed him. In the fall of that year he therefore began to teach mathematical astronomy, a field in which it would be highly desirable to have a competent knowledge of the physical astronomy contained in Aristotle's *De caelo*. Here Galileo's contact with Vallius probably stood him in good stead, for Vallius himself had passed from logic to the course on natural

philosophy at the Collegio, and was actually teaching the *Physics* and the *De caelo* in the academic year 1588—1589. Once Galileo had seen the thoroughness of Vallius's teaching notes for the *Posterior Analytics*, it would be natural for him to turn to Vallius for similar expositions of his teachings on the universe and the heavens. Galileo's motivation, on this accounting, would not be to get a position teaching Aristotelian philosophy, as Drake has conjectured; at the time he had no special competence in that area, whereas he had shown himself to be quite good at mathematics, only requiring improvement in the application of that discipline to astronomy to discharge his new duties properly.

If Galileo did contact Vallius for his teaching notes on the *De caelo* in the summer of 1589, a peculiar situation would have developed that might serve to explain the otherwise intractable 1580 dating associated with the biblical chronology found in MS 46, referred to above. At that time, as can be seen in Figure 1, Vallius would not yet have finished his course on *De caelo*, and so could not have sent his own notes to Galileo; he probably had at hand, however, a very good set of notes deriving from his predecessor, Antonius Menu. Menu, it will be remembered, covered *De caelo* in 1577—1578, at which time his lecture notes show very good agreement for 7 of the 8 paragraphs in Galileo's question [D] — the only paragraph missing being the last paragraph containing the now notorious biblical chronology.[41] But, as already mentioned, Menu also taught *De caelo* one last time, in 1580—1581. It seems quite possible that, when revising his lecture notes in 1580, he decided to fix the date of the origin of the universe and so added the chronology. If he did so, Galileo would have received a set of notes in which the "present time" would have been given as 1580, whereas he himself would have been appropriating them late in 1589 or in 1590, in accordance with the dating I have proposed above. Again, when improving his lecture notes in 1580, Menu could have added the introductory matter contained in Galileo's questions [A] and [B], thus accounting for presence of these two questions in the lectures of subsequent professors.

Before leaving the *De caelo* portion of MS 46, in light of the foregoing conjecture we may also inquire into the provenance of Galileo's questions [G] and [H], which, though having counterparts in Clavius's *Sphaera* of 1581, do not appear in subsequent Jesuit lectures on the *De caelo*. Years ago, when writing my commentary on MS 46, I wondered whether Clavius's textbook was the direct source of Galileo's

two questions, since there are copying errors in them that suggest derivation from a manuscript rather than from a printed source.[42] A study of the *rotulus* of professors at the Collegio now turns up an interesting possibility. Mutius De Angelis, as mentioned previously, taught the *Physics* and the *De caelo* three times in the period between Menu and Vallius, that is, in 1584—1585, 1585—1586, and 1586—1587.[43] In those three academic years, in what surely is more than a coincidence, Clavius did not teach mathematical astronomy at the Collegio, having as substitutes Richardus Gibbone in 1584—1585 and Franciscus Fuligati from 1585 to 1587. In Clavius's absence, and in light of the growing awareness at the Collegio that mathematical astronomy had important bearing on the matters treated in *De caelo*, it seems reasonable to suppose that De Angelis would abbreviate Clavius's treatment of the number and order of the heavenly spheres in his *Sphaera* and incorporate them into his own teaching notes. If De Angelis did this, they could have become a part of the materials in Vallius's possession, which he then would have passed on to Galileo. On this accounting not only questions [A] and [B] but also questions [G] and [H] became available after Menu's lectures of 1578, which explains why they do not show up in Table 10 before Galileo's appropriating them in MS 46.

DATING THE *DE GENERATIONE* PORTION OF MS 46

The structure of MS 46, as diagrammed in Table 4, points to serious problems about its completeness, that is, whether all the folios that made it up originally are still extant or whether a substantial number have been lost. The first part of the codex, up to and including folio 54, written on paper which Drake identifies as having a Pisan watermark, terminates with Galileo's question [L] on the animation of the heavens. As can be seen from the comparable treatments of *De caelo* in the extant lectures of Menu (Table 5), Vitelleschus (Table 7), and Rugerius (Table 8), much more material pertains to the matter of *De caelo* than that appropriated by Galileo in these fifty-odd folios. Of particular importance is the study of the elements from the point of view of their heaviness and lightness and the local motions consequent on these motive forces, which materials, although not treated by Menu, receive detailed attention from both Vitelleschus and Rugerius. In my commentary on MS 46 I established that Galileo's question [L] was com-

plete and self-contained, and thus that his note-taking on *De caelo* could have terminated at the bottom of the verso side of folio 54.[44] I tend, however, to agree with Favaro that the *De caelo* portion of MS 46, as it has survived to us, is incomplete, that at one time it probably contained discussions of the accidents of the heavens and how they act on the sublunary regions, topics that must have been of great interest to Galileo as he set out to teach the astronomy contained in the *Sphaera*. It also might have contained questions on *gravitas* and *levitas*, which would lead directly into the *De motu* treatises to be discussed below. This possibility, I admit, is more conjectural, since it turns on the precise exemplar available to Galileo — whether it contained only Menu's materials or, alternatively, substantial additions from De Angelis and Vallius that were later incorporated into the lectures of Vitelleschus and Rugerius.

However one resolves that question, there can be no doubt that the portions of MS 46 relating to the *De generatione* are incomplete: at the beginning, in the middle (between the folios presently numbered 74 and 75), and at the end. The material missing at the end is very important, for it is there, based on Menu's lectures, that one would expect to find Galileo's treatments of motive qualities and of natural and projectile motion, which would bear directly on the memoranda on motion in the third portion of MS 46 and on the various treatises on motion in MS 71. It is difficult to make a case from missing material, and so I have had to proceed cautiously in my previous work on this manuscript. When writing *Galileo and His Sources*, however, I devoted the whole of its fourth chapter to likely Jesuit counterparts of the missing materials.[45] The results are so concordant with Galileo's early treatises on motion that I am convinced that either Galileo appropriated in writing additional questions that are now lost, or, if not, that he worked over the Collegio exemplar so carefully that it became a mental part of his heritage and so exerted a substantial influence on his later writings.

Like myself and others, Drake locates the *De generatione* portion of MS 46 after the *De caelo* portion, and indeed separates them by a substantial interval of time. His main argument for doing so is that the folios on which the *De generatione* portion is written bear a Florentine watermark, different from the Pisan watermark on the *De caelo* portion. Since Galileo's lectures on Dante's *Inferno*, delivered at the Florentine Academy late in 1588, have the same Florentine watermark, Drake dates the *De generatione* portion in 1588 and holds that it too

was written in Florence. His general principle is that manuscripts bearing the same watermark "were composed at one place, from one stock of paper, around one period of time";[46] this principle he further applies to one of the treatises *De motu* in MS 71 and argues that it also was composed in Florence in 1588. I do not find this type of argument convincing. Florence and Pisa are not that distant apart, and Galileo traveled back and forth between them a goodly number of times; there is little reason to assume that on such trips he could not have carried small packets of paper along with him. Again, the phrase "one period of time" allows for considerable latitude; if he wrote the Dante lecture in late 1588 from a new stock of paper, he could well be using the same stock of paper through 1589 and even into 1590 and beyond.

Unlike Drake, moreover, I do not find it necessary to posit a long interval between Galileo's writing of the *De caelo* and the *De generatione* portions of MS 46. If there was such an interval, it was probably occasioned by Galileo's having to wait for Vallius to finish his latest lectures on *De generatione.* According to my calculations based on Figure 1, Vallius lectured on the *De generatione* in late 1585—early 1586, again in late 1586—early 1587, and for the last time in late 1589—early 1590. Assuming that Galileo had worked on MS 27 in early 1589, then continued with the *De caelo* portion of MS 46 in mid- or late 1589, he would not have gotten to the *De generatione* portion until late 1589 or early 1590, precisely when Vallius's latest teaching notes would have become available. And then, regardless of the watermarks on the paper he was using, he would have been at Pisa, teaching full time at the University.

This, to be sure, is not the only scenario one can excogitate. For example, if Vallius sent Galileo an exemplar of the *De caelo* lectures of one of his predecessors, such as Menu, it would probably have been a codex containing that predecessor's *De generatione* lectures also, since the materials are closely related and not infrequently are found in the same codex. Considering the many lacunae in the information available to us, particularly the unavailability of De Angelis's lectures on *De caelo* and *De generatione*, it is almost impossible to evaluate possibilities such as these. Yet one should not overlook the significance of the data that *are* available and that have been presented in Tables 9 and 10, for these show that the most significant correlations with Galileo's questions in MS 46 occur in the lecture notes of Vallius, Vitelleschus, and Rugerius. These are all fairly late, and thus are

discouraging for anyone wishing to situate the completion of MS 46 substantially earlier than 1589 or 1590.

Having mentioned Tables 9 and 10, let me finally call attention to the last entry in both tables, the name of Carbone, which yet further complicates the possibilities. Ludovico Carbone, it will be remembered, was the plagiarist who preserved the original exemplar behind Galileo's MS 27 and enabled me to fix the date of its composition. Not himself a Jesuit, Carbone had been a student at the Collegio and while there became a devoted admirer of his professors. He appears to have visited the Collegio frequently and was known to Jesuits there, Vallius, of course, included. In fact he paid them the great compliment of reproducing their teaching materials, particularly in the field of logic and rhetoric, with the apparently innocent intention of making them more widely available. My colleague at Catholic University, Professor Jean Dietz Moss, has been studying Carbone's writings on rhetoric and has been searching out manuscripts relating to them. While so doing, she recently came across a previously uncatalogued manuscript of Carbone in the Biblioteca Nazionale Centrale in Florence, which, surprisingly enough, summarizes lectures on the *De caelo* and *De generatione*.[47] With her permission I have transcribed the contents of the codex containing them, which is dated 1594, and present them in Table 11.

TABLE 11
Carbone: BNF CL XII, 64 Theatini (1594)

IN QUATUOR ARISTOTELIS LIBROS DE CAELO.	
Prolegomena in quatuor libros de Caelo	
De ordine horum librorum cum superioribus	[B]
De obiecto horum librorum	[A]
De inscriptione horum librorum . . .	[B]
Tractatus de universo	
De causis mundi deque eius definitione	[D]
De universi perfectione	[E]
De unitate mundi	[E]
An plures possint esse mundi	[E]
An hic mundus . . . non possit fieri perfectior	[E]
An Deus possit alios mundos perfectiores efficere	
Tractatus de caelo	
De ordine huius tractationis	
An caelum sit compositum ex elementis, an utrum sit quaedam quinta substantia	[I]

Table 11 (continued)

An coelum sit compositum ex materia et forma	[K]
An coelum sit animatum	[L]
An coelum sit corruptibile	[J]
An coelum sit perfectissium omnium corporum	
De [coeli] quantitate [continua]	
An in coelo sint differentiae positionum	
De numero et ordine coelorum	[GH]
De motu coelorum	
An coelum moveatur ab aliquo principio intrinseco . . . et ab aliqua Intelligentia vel a Deo	
Qualis sit motus coeli	
De quibusdam dubitationibus de natura stellarum	
Tractatus de actione coelorum in inferioribus	
An coelum agat in haec inferioria per motum	
An coelum agat per lumen, calefaciendo	
An cessante motu et influxu coeli, perirent haec inferiora	
An coelum agat in haec inferiora per influentias	
Tractatus de elementis	
An elementa dantur	[P]
De numero elementorum	[U]
De essentia elementorum	[Q]
De formis elementorum	[S]
De nobilitate, an ignis sit nobilior coeteris [elementis]	
De quantitate elementorum et de eorumque qualitatibus	[U]
De qualitatibus quantum ad earum sufficientiam . . .	
De harum qualitatum definitione	
De intensione et remissione harum qualitatum [motivarum]	
De termino istarum qualitatum	
De connexione et ordine qualitatum motivarum [et activarum]	
An qualitates motivae elementorum specie differant	
De applicatione istarum qualitatum ad ipsa elementa	
IN LIBROS DE GENERATIONE ARISTOTELIS	
Prolegomena in libros De generatione	
De obiecto horum librorum	
De divisione horum librorum	
Tractatus de generatione	
An detur generatio, an substantialis differat ab accidentali	
Quid sit generatio	
Quinam sit terminus generationis	
De subiecto generationis	
Quomodo causae secundae et particulares concurrant . . .	
Quomodo Deus concurrat ad actiones horum inferiorum	

Table 11 (continued)

Tractatus de corruptione	
An corruptio sit naturalis	
An generatio unius sit corruptio alterius	
An generatio et corruptio sint motus	
Tractatus de alteratione	
De nomine et definitione alterationis . . .	
Quid sit alteratio, et de termino et subiecto alterationis	[M]
Quomodo fiat intensio et remissio in qualitate	[N]
An alteratio sit motus continuus	[N]
Tractatus de actione	
Quid sit actio et quae de actione doceat Aristoteles	[W]
An omnis actio fiat per contactum	
An idem possit agere in se ipsum, et simile in simile	
An omne agens agendo repatietur	
Tractatus de mixtione	
De natura mixtionis	
An formae elementorum maneant in . . . parte mixti	[T]
Utrum qualitates elementorum maneant formaliter in mixto	[T]
Tractatus de quatuor qualitatibus	
An harum qualitatum duae sint activae et duae passivae	[X]
Quae harum qualitatum sit magis activa	[X]
An frigitas habeat vim producendi substantiam . . .	
An humiditas et siccitas sint ratio terminationis	
An humiditas et siccitas attingunt formam substantialem	
An istae qualitates sint primae in ratione agendi	[Y]
An quatuor qualitates primae sint primorum omnium actionum . . . in mixtis	
An singula elementa habeant binas qualitates primas vel aliquas secundas	
An qualitates simbolae sint eiusdem speciei	
An detur purum elementum	
An ex quoque elemento possit generari . . . elementum	
An in elementis simbolis sit facilior transitus . . .	
Utrum ex duobus elementis contrariis fiat aliquod unum tertium	

What is most significant about Carbone's notes is the extensive number of questions he has treated, for he covers in a general way most of the material in Galileo's MS 46 and much more besides. (It is to make this point that I show his correlations with MS 46 along with the other correlations presented in Tables 9 and 10.) As Moss has shown in her writings on Carbone, he was an excellent pedagogue and was

very good at summarizing and re-ordering teaching materials.[48] I have not had the chance to study the codex containing his notes carefully, but my preliminary impression is that they show few word-for-word agreements with Galileo's text, presenting in the main conclusions consonant with Galileo's but leaving out the detailed arguments on which they are based. For purposes here, this is not important. What is important is the type of editorial and diffusion activity in which Carbone was engaged. To illustrate the point, when I first began searching for manuscript sources of MS 46 I came across, in the archives of the Collegio Romano, a series of notes on the eighth book of Aristotle's *Physics* and the first two books of *De caelo* that bore significant resemblances to Galileo's questions.[49] The author was Placidus Carosus, a monk at a monastery in Perugia, and the codex was dated 1589. I often wondered how a monk in Perugia could have written questions on the *De caelo* that ended up in the Collegio's archives. Now the answer is clear: Carbone was himself for many years a professor in Perugia, and I have no doubt now that he was the intermediary through which Carosus gained access to the Jesuit lectures on which his notes were obviously based.

This instance prompts an interesting speculation relating to Vallius, Carbone, and MS 46 along lines similar to those of MS 27. Since Carbone was a frequent visitor to the Collegio Romano and was intent on diffusing Jesuit teachings, it is quite possible that he met Galileo during Galileo's visit to Rome late in 1587. If so, and particularly if Clavius had put Galileo in touch with Vallius before he left Rome, it could well be that Vallius-Carbone stand in a relationship to MS 46 analogous to their relationship to MS 27. Carbone then would have functioned as an intermediary between Vallius and Galileo for the materials of MS 46, just as he was for those of MS 27. The dating of Carosus's notes in 1589 lends support to this speculation, for it was precisely around this time that Galileo was interested in the very materials that had attracted the attention of both Carbone and Carosus.

MS 71: THE *DE MOTU* TREATISES

The possibilities, then, for the transmission of the materials in MS 46 are legion, and they are not less so for MS 71. The latter, as already noted, is even more complex because of its containing Galileo's original compositions, and thus the type of argument based on word-for-word

agreement that has been used heretofore is no longer available for dating purposes. There are clear connections, however, between MS 71 and MS 46, just as there are between MS 46 and MS 27, and when these are studied closely they also lead to plausible conclusions that are somewhat different from Drake's conclusions based on the study of watermarks. The main result to which they come is that MS 71 was written after MSS 27 and 46, in that order, and for the most part at Pisa, before Galileo left there in 1592 to begin his career at the University of Padua.

The key connector, already noticed by Drabkin and Fredette, are the memoranda on motion that occupy the last nine folios of MS 46.[50] These are jottings or excerpts from different sources that show up in various ways in the treatises on motion in MS 71. In my view they are closely connected with the folios that are missing from MS 46 and should be seen as a complement to the Jesuit questions on *gravitas* and *levitas*, on the natural motion of heavy and light bodies, and on projectile motion, with which most of the MS 71 treatises are concerned. Drake does not discuss the watermark on these folios, which is different from the watermarks on the *De caelo* and the *De generatione* portions of MS 46, but Crombie identifies it as "a ladder in a shield".[51] This turns out to be irrelevant for dating purposes, as Drake rightly observes, since the folios are pages from an old notebook apparently used by Galileo's younger brother.[52] Yet the memoranda were obviously written in chronological sequence, and thus by comparing them with their counterparts in the various documents making up MS 71 one can gain a fair idea of the order in which the documents themselves were composed.

If my chronology to date is correct, the memoranda were begun in late 1589 at the earliest, and more reasonably in 1590. At that time Galileo would again have been teaching mathematics at the University, covering the fifth book of Euclid's *Elements* and probably the *Theorica planetarum*, an astronomical treatise that usually followed the *Sphaera* he had taught in the previous academic year. It is noteworthy that Jacopo Mazzoni, Galileo's friend and collaborator,[53] was assigned to teach Aristotle's *De caelo* during the same academic year, so they both would have been concerned with similar subject matters. One might wonder, of course, why Galileo would become interested in *De motu* treatises at this particular time, when his main responsibility was mathematics and mathematical astronomy. But in this connection his

predecessor at Pisa, Father Fantonius, had himself written a treatise *De motu* while teaching mathematics there, so there was a precedent for this interest. Charles Schmitt has shown, moreover, that Father Fantonius had based his treatise heavily on the *De motu gravium et levium* of Hieronimus Borrus, one of Galileo's teachers at Pisa, of whom Galileo is critical in one of the treatises contained in MS 71.[54] These are all minor considerations, but they may help explain why, apart from the stimulation he may have received from the Jesuit notes, Galileo embarked on the enterprise of MS 71 at this time.

The structure of MS 71 is the following: it begins with a single folio containing a plan for *De motu*; then has a 32-folio dialogue on motion; then an incomplete 18-folio treatise on motion; then a complete 64-folio treatise on motion; and finally two folios containing variants of the first two chapters in the complete treatise. Between the dialogue and the incomplete treatise there is a four-folio insertion, *De motu accelerato*, obviously written later; also, between the complete treatise and the variants of its first two chapters, but bound in up-side down and backwards, there is a Latin translation of a Greek work by Isocrates — a residue no doubt of Galileo's classical training before entering the University of Pisa and clearly irrelevant to *De motu*.

Although Favaro was of the opinion that the dialogue on motion was the last piece written, all recent interpreters are agreed that it represents Galileo's first attempt at the subject and so should be dated earliest. The setting of the dialogue is in Pisa, and the main internal evidence available for dating is the Galileo's mention in it of his *bilancetta*, which he had invented in 1586, and of a Dionigius Fons, who is portrayed in the dialogue as living but is known to have died on December 5, 1590. The dialogue was also begun before the memoranda on motion, since the first entry in the memoranda is a revision of a portion of the dialogue. Considering that a number of points relating not only to local motion but also to the logic of proof reflect a knowledge of the Collegio materials,[55] I would date the dialogue in 1589 and see it as written at Pisa contemporaneously with the materials missing from the *De generatione* portions of MS 46. Drake, lacking watermark evidence that would tie its composition to either Florence or Pisa, locates it in Siena between 1586 and 1587[56]; I see this as too early, particularly in view of its revision in MS 46, which by my estimate could not have been written before 1589.

The remaining treatises on motion are much more problematical,

with their dating being a subject of dispute between Drake and Raymond Fredette.[57] In *Galileo and His Sources* I followed Fredette's dating, while acknowledging in a note that some elements of Drake's ordering fit in better with the Jesuit materials I was analyzing in that work.[58] Briefly put, the problem is whether the incomplete *De motu* preceded the complete *De motu* or was intended as a partial revision of it; related to that problem is where to locate the two-folio revision of the first two chapters. On the basis of watermarks Drake holds that the incomplete *De motu* was written in Florence in 1588, that the revisions were made in Pisa in 1590, and that the complete *De motu* was written at Pisa (and possibly Florence) in 1590—1591. Fredette, on the other hand, finding more parallels for the later memoranda in the incomplete *De motu*, sees it as being written later than the complete *De motu*, in which more parallels for the earlier memoranda are to be found; if the chronological composition of the treatises follows the sequence of the memoranda, as many of us have held, then Fredette has the better of the argument. The two-folio revisions, on the other hand, represent a consistent attempt on Galileo's part to remove *levitas* completely from the work and to replace *leve* by *minus grave*. For Fredette, therefore, the earlier versions had *gravitas* and *levitas* as two independent principles of natural motion whereas the later versions had *gravitas* only, merely assigning to it various degrees. Drake is forced to hold the opposite position, namely, that Galileo opted first for *gravitas* as a single principle and then returned to the Aristotelian insistence on both *gravitas* and *levitas* as dual principles of natural motion. The Jesuits argued the relative merits of both positions and came down on the side of two principles. Thus, if Drake is correct, there could be a more pronounced Jesuit influence in Galileo's treatise than has previously been recognized. Also, in support of Drake, I have recently examined a microfilm of MS 71 and find that the folios of what I have referred to as the complete *De motu* are numbered sequentially by Galileo in his own hand, a fact that has hitherto be unnoticed by scholars. Even though Galileo failed to number the chapter headings of the treatise, therefore, this is an indication that he felt he had in it a fully ordered account.

Others of Drake's speculations, however, do not fit so well with the Jesuit lecture materials. For him, the revisions and the complete *De motu* with its logical structure were inspired by Vallius's notes as appropriated in MS 27; the earlier dialogue and the incomplete *De*

motu, on the other hand, were not scholastic or Aristotelian, and thus not of Jesuit origin, but derived instead from Galileo's studies of specific gravity and his interest in Archimedes. Again, in his view the early versions of *De motu* had "a theological or metaphysical opening" that was removed in the later versions. As I see it, however, these speculations introduce a false dichotomy between Aristotelian and Archimedean science; both the earlier and the later versions conform to the methodological canons of the *Posterior Analytics*, as I have carefully illustrated in *Galileo and His Sources*.[59] Again, the Jesuits too were aware of specific gravity and Benedetti's analyses of local motion; their notes, plus Galileo's contacts with Mazzoni, accord equally well with Archimedean influences.[60] Yet again, a more theological cast is found in the *De caelo* lectures of Menu and Carbone than in those of Vitelleschus and Rugerius, and yet all are of Jesuit origin; the "theological opening", therefore, may reflect only a different set of notes from the Collegio or a different portion of the same set.

Finally, returning to the dialogue and its revision (the first item in the memoranda on motion), we note that even there Galileo could not make up his mind on whether *gravitas* and *levitas* were necessary to explain natural motion or whether *gravitas* alone might suffice. I am inclined now to believe that a certain ambivalence in his thought persisted to the end, and thus feel no need to decide between the alternative solutions offered by Drake and Fredette. According to my dating, apart from the dialogue most of the material contained in MS 71 was drafted within a year (or a year and a half at best), hardly sufficient time for a significant evolution to have taken place in Galileo's thought. Although he had made a considerable attack on the problem of motion while at Pisa, he himself was probably aware that he had not solved the problem definitively. The solution he sought would not come until his period of extensive experimental activity at Padua, out of which would come the *De motu accelerato* fragment, the cornerstone of the "new science" of motion that would be featured in the *Discorsi* of 1638.

Much more material pertaining to the analysis of MS 71 could be adduced, but limitations of space and time require me to call a halt at this point. Drake's study of Galileo's notes on motion from 1600 to 1636 yielded significant results, in my view, because he worked with a large number of folios with distinctive watermarks, written at well separated places and times, accompanied by dated letters that enabled

him to locate them spatio-temporally with a fair degree of precision. The same method, applied to a few documents written in the Florence-Pisa-Siena triangle over a relatively short period of time, can hardly be expected to yield similar results. But nonetheless Drake is to be congratulated in the attempt, for his elegant proposal has stimulated me, and I hope will stimulate others, to make more precise the dating and significance of Galileo's pre-Paduan manuscripts.[61]

NOTES

[1] Much of the research on which Drake's conclusions are based is summarized in his *Galileo at Work: His Scientific Biography*, Chicago-London: The University of Chicago Press, 1978.

[2] Stillman Drake, "Galileo's Pre-Paduan Writings: Years, Sources, Motivations", in *Studies in History and Philosophy of Science*, **17** (1986) 429—448, henceforth cited as "Pre-Paduan Writings".

[3] The principal results are recorded in my *Galileo's Early Notebooks: The Physical Questions*. A Translation from the Latin, with Historical and Paleographical Commentary, Notre Dame, Ind.: University of Notre Dame Press, 1977; *Prelude to Galileo: Essays on Medieval and Sixteenth-Century Sources of Galileo's Thought*, Dordrecht-Boston: D. Reidel Publishing Co., 1981; *Galileo and His Sources: The Heritage of the Collegio Romano in Galileo's Science*, Princeton: Princeton University Press, 1984.

[4] See the table on p. 440 of "Pre-Paduan Writings".

[5] "Pre-Paduan Writings", p. 448.

[6] Personal correspondence: Drake to Wallace, 16 March and 3 April 1985; Wallace to Drake, 26 March 1985.

[7] William A. Wallace, "Galileo's Sources: Manuscripts or Printed Works?" in *Print and Culture in the Renaissance*, eds. S. Vagonheim and G. Tyson, Newark, Del.: The University of Delaware Press, 1986; pp. 45—54; "The Early Jesuits and the Heritage of Domingo de Soto", in *History and Technology*, **4** (1987) 301—320; and "Science and Philosophy at the Collegio Romano in the Time of Benedetti", *Atti del Convegno Internazionale di Studio Giovan Battista Benedetti e il Suo Tempo*, Venice: Istituto Veneto di Scienze, Lettere ed Arti, 1987; pp. 113—126. *

[8] It is possible for a paleographer to differentiate between copying based on oral transmission and that based on a written exemplar; for an example of the former, see Joseph Cos, "Evidences of St. Thomas's Dictating Activity in the Naples Manuscript of his *Scriptum in Metaphysicam*, Naples, BN VIII F. 16", in *Scriptorium*, **38** (1984) 231—253.

[9] See my "Galileo's Sources: Manuscripts or Printed Works?" and *Prelude to Galileo*, pp. 194—217.

[10] Adriano Carugo and Alistair Crombie have consistently worked on the supposition that MSS 27 and 46 were based on printed sources, and thus arrive at datings for both manuscripts that differ widely from those arrived at by Drake and myself. Their work is cited in "Pre-Paduan Writings", p. 429, notes 2 and 3, and in note 51 below.
[11] This technique is described in detail in *Galileo's Early Notebooks* and *Galileo and His Sources.*
[12] The sequence was not followed invariably; sometimes, for example, the *Meteorology* and the *De generatione* were interchanged. For a general description of the course structure see R. G. Villoslada, *Storia del Collegio Romano dal suo inizio (1551) alla soppressione della Compagnia di Gesu' (1773)*, Rome: Gregorian University Press, 1954.
[13] A year-by-year tabulation of the professors and the courses they taught at the Collegio, to the extent that these are known, is given in *Galileo and His Sources*, p. 7.
[14] Msc. Class., Codd. 62-1 through 62-7.
[15] Galileo Galilei, *Tractatio de praecognitionibus et praecognitis* and *Tractatio de demonstratione.* Transcribed from the Latin Autograph by William F. Edwards, with an Introduction, Notes, and Commentary by William A. Wallace, Padua: Editrice Antenore, 1988.
[16] Ludovicus Carbone, *Additamenta ad commentaria D. Francisci Toleti in Logicam Aristotelis*, Venice: Apud Georgium Angellerium, 1597.
[17] Paulus Vallius, *Logica . . . duobus tomis distincta*, Lyons: Ludovicus Proust, 1622.
[18] A complete listing is given in *Galileo and His Sources*, pp. 30—32.
[19] See *Galileo and His Sources*, pp. 126—128, 147—148; also my commentary on Galileo's *Tractatio de demonstratione* (note 15 above), pp. 173—177.
[20] *Galileo and His Sources*, pp. 44—51.
[21] *Galileo and His Sources*, pp. 18—19.
[22] To facilitate reference, these are the same letters as were used in designating the questions in my English translation of MS 46 in *Galileo's Early Notebooks: The Physical Questions.*
[23] Fuller details are given in *Galileo and His Sources*, pp. 54—89.
[24] Rome, Universita' Gregoriana, APUG-FC Cod. 1710.
[25] *Galileo and His Sources*, p. 64.
[26] *Galileo and His Sources*, pp. 64—66.
[27] The last paragraph of Galileo's question [T] begins on the top of the recto side of folio 74 and is incomplete, the rest of the folio being blank on both sides. The first extant paragraph of his question [U], on the other hand, begins on the verso side of folio 75, the recto side of which is also blank. It is probable that at one time additional materials, since lost, were to be found between folios 74 and 75 in their present enumeration.
[28] Sommervogel records that De Angelis composed commentaries "on almost all the philosophical works of Aristotle"; I have searched extensively for these but without success. See *Galileo's Early Notebooks*, pp. 22—23.
[29] Any more than the extensive correlations of Galileo's MS 27 with Carbone's *Additamenta* is a sign of Galileo's having appropriated Carbone's questions, as Carugo and Crombie have thought; see note 10 above.
[30] While favoring this date, Drake also allows the possibility that these treatises were written in 1588, which, while still too early, would be more in accord with my calculations. See "Pre-Paduan Writings", pp. 436—437, but also note 32 below.

[31] *Prelude to Galileo*, pp. 217—225; *Galileo's Early Notebooks*, 258—259; *Galileo and His Sources*, pp. 89—95.
[32] Favaro's error would seem to have been the following. Instead of focusing on Galileo's last entry in the chronology, which gives the interval between the destruction of Jerusalem and "the present time" as 1510 years, Favaro went back one entry further, which records the interval between the birth of Christ and the destruction of Jerusalem as 74 years. Adding 74 to 1510, he thus came to A.D. 1584 as "the present time". Apparently unfamiliar with biblical chronologies, Favaro thought that the first year of our Christian era began with the birth of Christ — a reckoning that was made, incorrectly, by Dionysius Exiguus in the first part of the seventh century A.D. Already by Galileo's time, however, it was known that Dionysius's figures were incorrect; secular history revealed that Christ's birth could not have occurred later than 4 B.C., since that is the year in which Herod the Great died. Jesuit and other chronologers, including Galileo indirectly, thus put the interval between Christ's birth and the year in which Jerusalem was destroyed — a date also well fixed by secular historians — as 74 years. Drake's speculation that Galileo himself constructed the chronology and "made the calendrical adjustment of four years, but in the wrong direction" ("Pre-Paduan Writings", p. 437), lacks all plausibility. This, unfortunately, is the way in which he arrives at 1588 as a possible date for the writing of the *De caelo* portion of MS 46 (note 30 above), compounding Favaro's error by adding four more years to the latter's incorrect date of 1584.
[33] See the references given in note 31 above.
[34] "Pre-Paduan Writings", p. 347.
[35] The *rotulus* of professors and their course offerings are preserved in the Archivio di Stato, Pisa, Universita' G. 77, fols. 164v—194r, which I have consulted for this information; the relevant notation is on fol. 172r.
[36] *Galileo and His Sources*, pp. 91—95, 223—225.
[37] *Galileo and His Sources*, p. 18.
[38] All of these occurrences are noted in my commentary on Galileo's logical treatises (note 15 above, *ad indicem*). A possible sign of concern over the *petitio principii* on Galileo's part is detectable in his difficulties in writing this expression properly, all of which are noted in my commentary.
[39] *Galileo and His Sources*, pp. 90—91, 106—107.
[40] See note 15 above. Edwards' emendations are contained in the critical apparatus to his text, mine in the discussion of signs of copying and other paleographical features at the beginning of the commentaries on individual questions.
[41] *Galileo's Early Notebooks*, p. 42, par. D8; see also the commentary on par. D8, pp. 258—259.
[42] *Galileo's Early Notebooks*, commentary on paragraphs G1, G13, G17, H12, and J24, pp. 263—269.
[43] See Table 1 in *Galileo and His Sources*, p. 7.
[44] *Galileo's Early Notebooks*, comment on par. L41, p. 276.
[45] *Galileo and His Sources*, pp. 149—216.
[46] "Pre-Paduan Writings", p. 431.
[47] BNF Cod. CL XII, 64 Theatini.
[48] Jean Dietz Moss, "The Rhetoric Course at the Collegio Romano in the Latter Half of the Sixteenth Century", *Rhetorica* 4 (1986) 117—151; idem, "Aristotle's Four Causes: A Forgotten *Topos* of Renaissance Rhetoric", *The Rhetoric Society of America*

Quarterly **4** (1987) 71—88; and idem, "Rhetorical Invention in the Italian Renaissance", in *Visions of Rhetoric: History, Theory and Criticism*, ed. C. W. Kneupper, Arlington, Tex.: Rhetoric Society of America, 1987.

[49] Rome, Universita' Gregoriana, APUG-FC, Cod. 690.

[50] The memoranda have been translated by I. E. Drabkin in *Mechanics in Sixteenth-Century Italy*, eds. S. Drake and I. E. Drabkin, Madison: The University of Wisconsin Press, 1969.

[51] Crombie, A. C., "Sources of Galileo's Early Natural Philosophy", in *Reason, Experiment, and Mysticism in the Scientific Revolution*, eds. R. M. Righini Bonelli and W. R. Shea, (New York: Science History Publications, 1975), p. 305.

[52] "Pre-Paduan Writings", pp. 433—434, table on p. 440.

[53] That Galileo and Mazzoni were collaborating is clear from a letter written by Galileo to his father on November 15, 1590; see *Prelude to Galileo*, p. 227. Possible influences of Mazzoni on Galileo, and particularly the possibility of their joint study of Benedetti's work, are discussed in *Galileo and His Sources*, pp. 225—230.

[54] See Essay 10, entitled "Filippo Fantoni, Galileo Galilei's Predecessor as Mathematics Lecturer at Pisa", in C. B. Schmitt's *Studies in Renaissance Philosophy and Science*, (London: Variorum Reprints, 1981). The essay originally appeared in *Science and History: Studies in Honor of Edward Rosen*, published at Wroclaw in 1978, pp. 53—62.

[55] *Galileo and His Sources*, pp. 230—235.

[56] "Pre-Paduan Writings", p. 440.

[57] See the bibliography cited in *Galileo and His Sources*, pp. 230—231 at notes 22 and 24; also pp. 357—358.

[58] Note 24 on p. 231.

[59] *Galileo and His Sources*, pp. 235—248.

[60] *Galileo and His Sources*, pp. 168—172, 184—202. The Jesuit philosophers may not have known of Benedetti directly, but they do cite Jean Taisnier's plagiarism of his work, as I note on p. 185; there is no doubt that Clavius was acquainted with him, as I show in my paper on Benedetti and the Collegio Romano, note 7 above. On the
* contracts with Mazzoni, see note 53 above.

[61] Work on this essay was supported by a grant from the National Endowment for the Humanities, RL-21080-87. A further elaboration will appear in a volume the author is
* preparing under the auspices of the grant with the projected title: *Galileo's Logical Methodology*, with a translation of, and commentary on, *His Appropriated Treatises* on Aristotle's *Posterior Analytics.*

XI

Galileo's Pisan studies in science and philosophy

The aura surrounding Galileo as founder of modern science disposes many of those writing about him to start *in medias res* with an account of his discoveries with the telescope, or with his dialogues on the world systems and the two new sciences, or with the trial and the tragic events surrounding it. Frequently implicit in such beginnings is the attitude that Galileo had no forebears and stands apart from history, this despite the fact that he was forty-six years of age when he wrote his *Sidereus Nuncius* and then in his late sixties and early seventies when he composed his two other masterpieces.

Attempts have recently been made by scholars to dispel this myth by giving closer scrutiny to the historical record – closer, that is, than one gets from perusing the National Edition of Galileo's works.[1] This was a masterful collection, but begun as it was in the last decade of the nineteenth century and completed in the first decade of the twentieth, it perforce could not benefit from the historiographical techniques developed in our century. During the past twenty years, in particular, much research has been done on Galileo's manuscripts, and it sheds unexpected light on what has come to be known as Galileo's "early period" – that covering the first forty-five years of his life.[2] This period has been singularly neglected by historians, and to their disadvantage, if the adage *parvus error in initio magnus in fine* may be applied to the history of ideas.

PERSONS AND PLACES IN TUSCANY

Galileo's father, Vincenzio Galilei, was born in Florence in 1520 and flourished there as a teacher of music and a lutanist of ability (Drake 1970). Having studied music theory for a while with

Gioseffo Zarlini in Venice, he married Guilia Ammannati of Pescia in 1563 and settled in the countryside near Pisa. There their first child, Galileo Galilei, was born on February 15, 1564. The family returned to Florence in 1572, but the young Galileo was left in Pisa with a relative of Guilia by marriage, Muzio Tedaldi, a businessman and customs official.

Two years later, Galileo rejoined his family in Florence and was tutored there by Jacopo Borghini until he could be sent to the Camaldolese Monastery at nearby Vallombrosa to begin his classical education. While at that monastery, Galileo was attracted to the life of the monks and actually joined the order as a novice. Vincenzio was displeased with the development, so he brought his son back to Florence where he resumed his studies at a school run by the Camaldolese monks but no longer as a candidate for their order.

Vincenzio's plan for Galileo was to become a physician, following in the footsteps of a fifteenth-century member of the family, also named Galileo, who had achieved great distinction as a physician and also in public affairs. Accordingly, he arranged for his son to live again with Tedaldi in Pisa and had him enrolled at the university there as a medical student in the fall of 1581 (Drake 1978).

The next four years of his life Galileo spent at the University of Pisa, studying mainly philosophy, where his professors were Francesco Buonamici and Girolamo Borro, and mathematics (including astronomy) under a Camaldolese monk, Filippo Fantoni. He probably went back to Florence for the summers, however, and this provides a key to the way Galileo supplemented the instructions he received in mathematics from Father Fantoni.

It was the custom of the Tuscan court to move from Florence to Pisa from Christmas to Easter of each year, and the court mathematician at the time was Ostilio Ricci, a competent geometer who is said to have studied under Niccolò Tartaglia (Settle 1971, Masotti 1975). During the 1582–1583 academic year, Galileo met Ricci while the latter was at Pisa and sat in on lectures Ricci was giving on Euclid to the court pages.

The following summer, when Galileo was back home, supposedly reading Galen, he invited Ricci to meet his father. Vincenzio was impressed with Ricci and the two became friends. Ricci told Vincenzio that his son was little interested in medicine, that he wanted to

become a mathematician, and sought permission to instruct him in that discipline. Despite Vincenzio's unhappiness with this request, Galileo was able to avail himself of Ricci's help and devote himself more and more to the study of Euclid and Archimedes, probably with the aid of Italian translations prepared by Tartaglia.

By 1585, Galileo dropped out of the University of Pisa and began to teach mathematics privately at Florence and at Siena, where he had a public appointment in 1585–1586, and then at Vallombrosa in the summer of 1585. In 1587, Galileo traveled to Rome to visit Christopher Clavius, the famous Jesuit mathematician at the Collegio Romano. And in 1588, he was invited to the Florentine Academy to give lectures on the location and dimensions of hell in Dante's *Inferno*.

In 1589, Fantoni relinquished the chair in mathematics at Pisa and Galileo was selected to replace him, partly because of the favorable impression he had made on the Tuscan court with his lectures on Dante and partly on the recommendation of Clavius and other mathematicians who had become acquainted with his work. Galileo began lecturing at Pisa in November 1589, along with Jacopo Mazzoni, a philosopher who taught both Plato and Aristotle and was also an expert on Dante, and the two quickly became friends (Purnell 1972, DePace 1993).

Mazzoni is of special interest because of his knowledge of the works of another mathematician, Giovan Battista Benedetti, and because he is given special mention by Galileo in a letter from Pisa addressed to his father in Florence and dated November 15, 1590. In it, Galileo requests that his seven-volume Galen and his *Sfera* be sent to him at Pisa and informs his father that he is applying himself "to study and learning from Signor Mazzoni," who sends his regards (EN10:44–5).

Galileo then taught at the University of Pisa until 1592, when financial burdens put on him as the eldest son at the death of his father in 1591 required him to obtain a better salary than the 60 florins he was being paid. He sought and received an appointment at the University of Padua at a salary of 180 florins, where he delivered his inaugural lecture on December 7, 1592.

He spent the next eighteen years in the Republic of Venice, which he later avowed were the happiest years of his life. Then he returned

to the Florentine court in 1610 as mathematician and philosopher to Cosimo II de' Medici, the Grand Duke of Tuscany.

MANUSCRIPTS AND THE EXPANDED DATA BASE

We have touched on places and persons in Tuscany that played a significant role in Galileo's intellectual development. The principal locations are Pisa and Florence, with Vallombrosa and Siena of secondary importance, along with the outside trip to Rome, which fortunately gave rise to materials that greatly enlarge the data base on which we can work. Galileo left a number of manuscripts dating from about 1580 to 1592, most in his own hand and in Latin, much of it on watermarked paper. Antonio Favaro transcribed some of the manuscripts for the National Edition and made a few notations regarding Galileo's peculiar spelling of Latin terms.

He also was able to identify two sources Galileo used for note taking, both translations of Plutarch's *Opuscoli Morali*, one published at Venice in 1559 and the other at Lucca in 1560 (EN9:277–8). Apart from this, Favaro could only conjecture about Galileo's sources and the periods during which he composed the various manuscripts that make up his Tuscan heritage, most of which are still conserved in Florence's Biblioteca Nazionale Centrale.

Serious work on these materials began around 1970, when Stillman Drake worked out a technique for dating Galileo's manuscripts through a study of the watermarks on the paper on which they were written and when other scholars, myself included, began to uncover the source materials on which the natural philosophy contained in one of the manuscripts was based.[3] Over the past twenty-five years, this research has expanded to include full studies of watermarks (Camerota 1993), detailed paleographical studies of Galileo's handwriting and word choice (Hooper 1993), and analyses of the ink he used when writing the manuscripts (Hooper 1994).[4]

Research on the sources of Galileo's philosophy proved particularly fruitful, since it turned out that a large part of that philosophy was appropriated from notes of lectures given in Rome by Jesuit professors of the Collegio Romano – the prestigious university established in that city by the founder of the Jesuits, Ignatius Loyola. Although Galileo did not attend those lectures, he somehow obtained copies of them and then appropriated selected materials for

his own use. Since the Jesuit notes can be dated, the discovery in them of passages with correspondences in Galileo's writings offers an additional way to determine the time and place of Galileo's compositions.

The manuscripts most important for this enterprise are all in Galileo's hand and are four in number. One is a special collection, *Filza Rinuccini* 2, and contains Galileo's lectures on Dante's *Inferno*; this was given in Florence and is written on paper bearing a Florentine watermark. The other three are in the group of manuscripts at the Biblioteca Nazionale entitled *Manoscritti Galileiani* and bear the numbers 27, 46, and 71.

Manuscript 27 is labeled *Dialettica*, the term used for the whole of logic in Galileo's day, and contains two treatises on logic. Antonio Favaro regarded this as a "scholastic exercise" of Galileo and only transcribed its titles and a sample question in the National Edition (EN9:275–82). It gives many indications of having been copied or appropriated from one or more sources, and many of its folios bear watermarks, all of Pisan origin.

Manuscript 46 bears the notation that it contains "an examination of Aristotle's *De caelo* made by Galileo around the year 1590" (EN1:9). This manuscript is essentially a notebook and it contains five treatises on different subjects, which Favaro transcribed and published in their entirety under the title *Juvenilia*, regarding it as a youthful composition (EN1:15–177). It, too, shows signs of copying, and its folios bear a variety of watermarks, most of either Pisan or Florentine origin.

Manuscript 71 differs from the other two in that there are cross-outs and emendations in the manuscript but no signs of copying. It apparently contains original drafts of essays by Galileo on the subject of motion; on this account, is referred to as the *De Motu Antiquiora*, the "older" science of motion, to distinguish it from the "new" science of motion published by Galileo in 1638. The folios of this manuscript, like the others, bear watermarks, a majority from Pisa but a significant number from Florence. Favaro also transcribed and published this manuscript (EN1:251–408), but in so doing he changed the ordering of the essays as they occur at present in the manuscript.

There are errors of Latinity in some of the noted manuscripts and also peculiarities of spelling. There are also internal references that

serve to show temporal connections between them. And, finally, there is now a substantial collection of possible source materials, some in print, others still in manuscript, on which Galileo could have drawn when writing them. Evaluating all of this material is the task one must face when trying to assess Galileo's intellectual formation. This took place mainly at the University of Pisa, but it was an ongoing process during the entire Tuscan period, prior to Galileo's move to the Veneto in 1592.

GALILEO'S APPROPRIATION OF JESUIT LEARNING

Of the material surveyed thus far, the most surprising is that associated with the Jesuits of the Collegio Romano, a source completely unsuspected for over four centuries.

I started my research on that subject at about the same time Drake was beginning his work on watermarks and have reported my findings in publications since then, principally 1981, 1984a, 1990, and 1992a, b. The path was tortuous and need not be reviewed here. The main conclusions were that the two manuscripts with the closest connections to the Jesuits, 27 and 46, were both composed at Pisa, the first in early 1589 and the second in late 1589 or early 1590 (Wallace, 1992b:39, 57).

The logic notes of manuscript 27 consist of two treatises relating to Aristotle's *Posterior Analytics*, one dealing with foreknowledge required for demonstration and the other with demonstration itself. Both treatises clearly derive from a course taught by Paulus Vallius in Rome, which did not conclude until August of 1588, and from which Galileo could not have appropriated his version until early 1589. Nothing in the watermark evidence and that derived from peculiarities in spelling alters this conclusion.

The situation is more complex with regard to Manuscript 46, labeled *Physical Questions* (Wallace 1977) to differentiate them from the *Logical Questions* of Manuscript 27. This is composed of three parts, the first containing portions of a questionary on Aristotle's *De caelo*, the second portions of a questionary on Aristotle's *De generatione*, and the third of series of memoranda on motion that are related to the composition of Manuscript 71, to be considered later. There are three treatises in the part pertaining to *De caelo*, the first

concerning the subject of that work, the second on the universe as a whole, and the third on the heavens.

All three of these are written on paper with Pisan watermarks and show few peculiarities of spelling. Since they presume knowledge of the logic contained in Manuscript 27 and show signs of improved Latinity, their writing is best located at Pisa around 1590, within a year after the questions on logic. The particular Jesuit set of notes Galileo used for his appropriation is not known with certainty, but a good possibility is that taught by Antonius Menu on *De caelo* in 1580. This source clears up a problem in the dating of Manuscript 46 based on the chronology given in it by Galileo (Wallace 1977:42, 258–9) and otherwise fits in with considerations presented in Wallace (1981:217–28) and Wallace (1984a:89–96).[5]

The second part of Manuscript 46 contains three treatises pertaining to *De generatione*, the first on alteration, the second on the elements, and the third on primary qualities. These are written on paper different from the first part, with Florentine watermarks, and they contain irregularities in spelling. The irregularities relate to word forms that are written differently in Italian and Latin, as, for example, *santo* and *sancto*, and occur in words with letter grouping like *-nt-* and *-st-*. Thus for *elementum* Galileo will sometimes write *elemenctum*; for *contra*, *conctra*; for *momentum*, *momenctum*; for *distantia*, *dixtantia*; and so on. These variants have been studied by Wallace Hooper (1993) who sees them as evidence of Galileo's learning when, and when not, to insert a *c* or an *x* when changing from an Italian to a Latin spelling.

Apparently, Galileo overcompensated at first and inserted too many *c*'s or changed an *s* to an *x* too often, for these forms quickly disappear in his later compositions. Their presence, therefore, is a good indication that their author, who had been accustomed to writing in Italian, was beginning to write in Latin as he prepared himself for an academic career. On the basis of this evidence it seems likely that these treatises were written in Florence and at a date even earlier than Manuscript 27, probably 1588.

Which of the Jesuit courses Galileo used for his appropriation is difficult to decide, but the best candidate is that on *De generatione*, offered in Rome by Paulus Vallius, the same Jesuit whose logical questions were used by Galileo when writing his Manuscript 27.

Unfortunately, the exemplar of Vallius's work on the elements that shows close correspondences with Galileo's Manuscript 46 is found in a codex that is undated. We do know, however, that Vallius taught *De generatione* there in 1585, 1586, and 1589, and, of these, the 1586 version would fit best with the new evidence.

As I have argued in Wallace (1984a:91–2, 223–5), Galileo first gained access to all these lecture notes through his visit to Christopher Clavius in 1587. At that time, he left with Clavius some theorems he had composed on the center of gravity of solids. In correspondence between the two in 1588, which involved Guidobaldo del Monte also, Clavius questioned Galileo's proof of the first theorem on the grounds that it contained a *petitio principii* (EN10:24–5, 29–30).

Since this type of question pertains to the foreknowledge required for demonstration, and at that time Vallius was teaching the part of the logic sequence dealing with foreknowledge and demonstration, it seems reasonable to suppose that Clavius would have put Galileo in touch with Vallius and that the latter would have made his lecture notes available to the young mathematician. Also, Galileo could well have had queries for Clavius on *gravitas* and *levitas* as these pertain to the elements, and Vallius would again be the best resource to whom Galileo could turn for information on these topics. This would explain how Galileo obtained not only the materials on which Manuscript 27 were based but also how the earlier version of Vallius's *De Elementis* (say, that of 1586) came to be incorporated in his Manuscript 46.

From the point of view of philosophy, Galileo's Manuscript 27 contains some very sophisticated information on scientific methodology, especially on the use of suppositions in scientific reasoning and on the role of resolution and composition as employed in the demonstrative *regressus*. Scholars have tended to overlook the regress, a powerful method of discovery and proof developed at the University of Padua, which reached its perfection in Galileo's lifetime (Wallace 1995). These areas of logic have been described in detail in my study of Galileo's sources (Wallace 1984a: Chapters 3, 5, and 6), which documents the recurrence of expressions found in Manuscript 27 in all Galileo's later writings. The implications of these logical teachings are more fully delineated in my examination of Galileo's logic of discovery and proof (Wallace 1992a), the first part of which (*logica docens*, Chapters 1–4) systematically analyzes the logic contained in

his logical treatises and the second part (*logica utens*, Chapters 5–6) how he used it in his works on astronomy and mechanics.

Manuscript 46 is almost four times longer than Manuscript 27, being composed of 110 folios as opposed to the latter's 31. Its material content covers the universe, the celestial spheres, and the elemental components of the terrestrial region, topics that engaged Galileo's attention throughout his life.

Two of its questions on the celestial spheres are clearly extracted from Clavius's commentary on the *Sphere* of Sacrobosco, either the 1581 or the 1585 edition. They show that Galileo was acquainted with Copernicus's teaching on the number and ordering of the spheres, even though he there defended the Ptolemaic teaching. He continued to teach Ptolemaic astronomy until the early 1600s, as is seen in his *Trattato della Sfera*, student copies of which were prepared from an original in Galileo's own hand between 1602 and 1606. The autograph has been lost, but Drake speculates that it was begun as early as 1586–1587, in conjunction with Galileo's private teaching of astronomy (Drake 1978:12). More likely, it was composed toward the end of 1590, when he wrote to his father requesting that his copy of the *Sfera* be sent to him at Pisa (*Sfera* here meaning the text with Clavius's commentary), and when he was writing the *De caelo* portion of Manuscript 46 containing the extract from Clavius (Wallace 1983, 1984a:255–61).

A striking but often unnoticed feature of Galileo's thought is his extraordinary grasp of Aristotelian teaching and his ability to engage the Peripatetics of his day on fine points of their interpretations. Such knowledge was not simply intuited by Galileo; he had to work to acquire it. He himself wrote to Belisario Vinta on May 7, 1610, when seeking the title of philosopher be added to that of mathematician to the Grand Duke of Tuscany, that he had "studied more years in philosophy than months in pure mathematics" (EN10:353). Surely the study and laborious appropriation of these lecture notes from Collegio Romano, a major portion of which is found in Manuscript 46, is to be counted among the "years in philosophy," to which Galileo there refers. As far as his use of the Jesuit questionaries on *De caelo* and *De generatione* is concerned, these have been partially investigated in my translation of Manuscript 46 (Wallace 1977:253–314) and more fully in later works (Wallace 1981, 1984a, 1991, and 1992a).

Galileo's formal study of philosophy, of course, took place at the University of Pisa from 1581 to 1585, and he had further contacts with the philosophers there when teaching mathematics at the university between 1589 and 1592. Possibly because Galileo later voiced his disagreement with the views of his teachers at Pisa, scholars have tended to undervalue his philosophical training there.

This may prove to be a mistake, since a number of studies are now available that connect his studies at the university with the manuscripts we have already discussed, as well as with Manuscript 71, which will occupy our attention in the following section. To lay the groundwork for that exposition, we now sketch the philosophical ambience at Pisa, with particular reference to Francesco Buonamici, Girolamo Borro and his influence on Filippo Fantoni, and Jacopo Mazzoni and the way in which he may have put Galileo in contact with the thought of Giovanni Batista Benedetti.[6]

Correspondences between the contents of Manuscripts 46 and 71 and the teachings of Buonamici have long been recognized and have been analyzed in some detail by Alexandre Koyré (1978). More helpful for our purposes is Mario Helbing's (1989) study of Buonamici's philosophy. This provides the complete background of Galileo's studies at Pisa, a full analysis of the contents of Buonamici's *De Motu*, and valuable reflections on his relations with Galileo. Helbing calls attention to the fact that the *De Motu* was already completed by 1587, though it was not published until 1591. Its importance derives from the fact that it records the fruits of Buonamici's teaching at the University of Pisa, where he taught natural philosophy from 1565 to 1587. His occasion for putting out the volume was, in Buonamici's own words, "a controversy that had arisen at the university among our students and certain of our colleagues on the motion of the elements" (Helbing 1989:54).

To appreciate the import of this statement one must be aware, Helbing points out, that professorial lectures were not the only means of transmitting knowledge to students at the time. Disputations were an additional component, and many of these seem to have centered on precisely the problems that interested Galileo. It could well be, therefore, that Galileo was one of the students to whom Buonamici refers. The colleagues mentioned most certainly include Borro, who

published a treatise on the motion of heavy and light bodies in 1575, to which Galileo refers in Manuscript 71, and probably Fantoni, who left a manuscript on the same subject that shows Borro's influence.

Helbing's thesis is that Buonamici's teaching exerted a substantial influence on the young Galileo, so much so that his own writings reflect a polemic dialogue with his teacher that continued through the years. The subjects and problems that preoccupied him were all contained in Buonamici's massive treatise, whose technical terminology Galileo took over as his own, even though his investigations led him to markedly different results.

Buonamici's project was to write a definitive treatise on motion in general that would explain its many manifestations in the world of nature on the basis of philological and scholarly research. Galileo's project, by way of opposition (EN1:367), was to concentrate on only one motion, essentially that of heavy bodies, and to make a detailed study of that using mathematical techniques to reveal its true nature. In his lectures, Helbing argues, Buonamici probably introduced Galileo to the atomism of Democritus, to Philoponus's critiques of Aristotle's teachings, to Copernicus's innovations in astronomy, to Archimedes and his use of the buoyancy principle to explain upward motion, to Hipparchus's theory of impetus, and to the writings of many others, including those of Clavius and Benedetto Pereira at the Collegio Romano – references to all of which can be found in his *De Motu*.

Galileo, without doubt, explicitly rejected many of Buonamici's teachings. Helbing notes that this rejection is particularly evident in Galileo's early writings, where Buonamici's arguments against Archimedes are definitely his target. Galileo also makes references to his former teacher in terms that are far from complimentary, in both the *Two Chief World Systems* (EN7:200, 231–2) and the *Two New Sciences* (EN8:190).

But despite these negative reactions, Helbing also records several areas of substantial agreement between Buonamici and Galileo, two of which are relevant to our study. The first is the general methodology they employ in their study of motion. Both wish to use a *methodus* to put their science on an axiomatic base, imitating in this the reasoning processes of mathematicians (*De Motu* 3A–B). Both regard sense experience as the foundation of natural science, taking this in a sense broad enough to include experiment in the rudimentary

form it was then assuming at Pisa. And both see causal reasoning and demonstration, with its twofold process of resolution and composition, as the normal road to scientific conclusions.

The second and more important area of agreement is the status each accords to mathematics both as a science in its own right and as an aid in investigating the secrets of nature. Buonamici lists the three speculative sciences as physics, mathematics, and metaphysics, and he insists that students should begin their study with mathematics, then proceed to physics, and ultimately to metaphysics.

Again, mathematics for him is the discipline that can raise one to divine science. It is also a true science that satisfies the requirements of the *Posterior Analytics*, against the teachings of Pereira, whom he cites explicitly. Its demonstrations are not limited to reductions to the impossible but include ostensive demonstrations of all three types: of the fact, of the reasoned fact, and, most powerful, making it the most exact of the human sciences. Buonamici further accords validity to the middle sciences (*scientiae mediae*) which he lists as optics, catoptrics, harmonics, astronomy, navigation, and mechanics, and he sees them as valuable adjuncts for the study of nature. This part of Buonamici's instruction seems to have deeply influenced Galileo and set him on the course that would bring him ultimately to Clavius and the Collegio Romano.

Two additional professors at Pisa, Borro the philosopher and Fantoni the mathematician, seem to have had less positive influence on Galileo. Borro was the type of philosopher against whom Galileo reacted most violently. Very different from Buonamici, he took most of his knowledge of Aristotle from medieval authors, especially Averroes in Latin translation. His writings show him much opposed to Platonism and the attempts being made in his day to reconcile Aristotle's ideas with those of his teacher.

Borro's anti-Platonism, coupled with his attraction to Averroes, are further revealed in his vehement rejection of mathematics and of the use of mathematical methods in the study of nature. He focused instead on the empirical side of Aristotelian philosophy, stressing the importance of observation and experience in uncovering the secrets of nature, and in this respect he undoubtedly exerted an influence on Galileo. This influence is seen in Manuscript 71, where Galileo shows his acquaintance with an experiment performed by Borro and described by him in *De Motu Gravium et Levium* (1575).

Fantoni is important for two *quaestiones* he left in manuscript form, one on the motion of heavy and light bodies, the other on the certitude of the mathematical sciences. His *De Motu* is of some significance for the fact that he wrote it not as a philosopher, as did Borro and Buonamici, but while teaching mathematics, and in so doing set a precedent for Galileo to prepare a similar treatise when he took over Fantoni's post. Actually, it presents little more than the kind of Averroist analysis found in Borro's book. The treatise on mathematics is also unoriginal, taking up positions similar to those defended by Buonamici in his massive text. What is noteworthy about it is that it is explicitly directed against Pereira. Fantoni argues that mathematics is a true science, that it fills all the requirements of the *Posterior Analytics* for certain knowledge, that it demonstrates through true causes, and that it can even achieve demonstrations that are most powerful – conclusions consonant with those of Clavius and the mathematicians at the Collegio Romano.

Possibly the strongest influence on Galileo from his years in Pisa, however, came not from his professors there, but from the colleague he encountered when he started teaching there, Jacopo Mazzoni. In 1590, when Galileo told his father that he was studying with Mazzoni, he was probably composing the notes on *De caelo* and *De generatione*, a course Mazzoni had taught the previous year.

Unlike his Pisan colleagues in philosophy, Mazzoni was not a monolithic Aristotelian. He also had Platonic sympathies, and in the summer of 1589 he had introduced a course in Plato's thought at the university. One of his major interests was comparing Aristotle with Plato, for he had made a concordance of their views in an early treatise published at Cesena in 1576. His major work on that subject, the *Praeludia*, did not appear until 1597, but there are indications Mazzoni was working on it over the intervening years. After its publication at Venice, in fact, Galileo wrote to him and remarked how their discussions at the beginning of their friendship were detectable in its composition (EN2:197).

Like Buonamici, Mazzoni takes a favorable view of the "mixed sciences," the *scientiae mediae*, and is explicit that Ptolemy's work pertains to that genre and also the work of Archimedes. It was Aristotle's shunning the use of mathematical demonstrations in physics, Mazzoni states, that caused him to err in his philosophizing about nature.

As an example, he cites Aristotle's teaching on the velocity of falling bodies. In detailing its particular errors and how they can be corrected, he turns to the work of Benedetti and particularly the way the latter used Archimedian principles to rectify Aristotle's teachings. Mazzoni's own treatment of the velocity problem, it turns out, more resembles that given by Galileo in Manuscript 71 than it does Benedetti's. This gives reason to believe that it was precisely these matters that Galileo and Mazzoni were studying late in 1590, the period during which it is commonly agreed Galileo was working on his *De Motu Antiquiora*.

Another comparison made by Mazzoni comes from his interest in pedagogy and concerns the relative merits of Plato and Aristotle for removing impediments encountered in the study of nature. Galileo discusses such impediments in his early writings and the various suppositions one may use to circumvent them. It is not unlikely that his studies with Mazzoni were seminal also in this respect.

With regard finally to Benèdetti's work on falling motion, Koyré suspected a connection between it and the positions taken in Manuscript 71 but had little textual evidence for it, since Galileo nowhere makes any mention of Benedetti. In particular, the anti-Aristotelian tone Galileo adopts in his Manuscript 71 resonates strongly with the tone of Benedetti's major work on falling motion, *Diversarum Speculationum Mathematicarum et Physicarum Liber*, printed at Turin in 1585.

Since this was available before 1590 and figures prominently in Mazzoni's *Praeludia*, it seems reasonable to suppose that Benedetti's text was itself the object of Galileo's study with Mazzoni referred to in the letter to his father. As I have pointed out elsewhere (Wallace 1987), Benedetti's basic disagreement with Aristotle was over the latter's not using mathematical principles and methods in the study of nature, a theme recurring in both Mazzoni and Galileo. Benedetti's work likewise abounds in suppositions and thought experiments, many of which are similar to Galileo's, and he, like Galileo, is particularly intent on discovering the causes of various properties of local motion – what they both call the *verae causae*, the true causes, as opposed to those proposed by Aristotle.

Information gleaned from the philosophical ambience at Pisa thus complements the materials contained in Manuscripts 27 and 46 and provides a fuller understanding of Galileo's intellectual development

during his years at Pisa. His interest in Archimedes undoubtedly dates from his studies with Buonamici and Ricci, the latter particularly because he helped Galileo hone his argumentative skills against his former teacher. His respect for Plato and his privileging Plato over Aristotle in some of his writings are at least partially explicable in terms of his contacts with Mazzoni (DePace 1992; Dollo 1989, 1990).

Nor does this type of influence from Mazzoni work at cross purposes with the materials Galileo appropriated from the Collegio Romano. In some matters, the Jesuits actually preferred Platonic teachings to those of Aristotle. For, as Crombie (1977) has amply demonstrated, they saw Platonism as fostering interest in the study of mathematics – which Calvius by 1589 had succeeded, over the objections of Pereira, in making a part of the *Ratio studiorum* at the Collegio Romano.

THE ARCHIMEDEAN – ARISTOTELIAN STUDY OF MOTION

This brings us back to Galileo's Manuscript 71 and his first sustained attack on the problem of falling motion, where, like Benedetti, he hoped to correct Aristotle with the aid of Archimedes. This manuscript has a number of components and the problem of ordering and dating these, partially explored by Favaro (EN1:245–9), has been the subject of renewed research on the basis of the new clues they present (Fredette 1972, 1975; Drake, 1986; Wallace, 1990; Camerota, 1993; Hooper 1993). We first review this development and then assess its import for the subsequent development of Galileo's science. The memoranda or jottings that Galileo made in preparation for his *De Motu* are found at the end of Manuscript 46, after the treatise on the elements, and this serves to tie the contents of Manuscript 71 to the physical questions.

These aside, the components of Manuscript 71 pertaining to the early *De Motu* are five in number and in the following order: a plan for the treatise, a dialogue on motion, a ten-chapter treatise on motion, a twenty-three-chapter treatise on motion, and variants of the first two chapters. In transcribing and publishing these, Favaro rearranged them, and the memoranda, in an order different from their appearance in the manuscripts, as can be seen from the following

listing, which shows the foliation of the manuscripts on the left and the pagination of the National Edition on the right:

MS 46 102r–110v	Memoranda	EN1:408–17
MS 71 3v	Plan for *De Motu*	EN1:418–9
MS 71 4r–35v	Dialogue on motion	EN1:367–408
MS 71 43r–60v	10-chapter treatise	EN1:344–66
MS 71 61r–124v	23-chapter treatise	EN1:251–340
Ms 71 133r–134v	Variants of first two chapters	EN1:341–3

Inserted into this material and occupying folios not listed above, are two items which Favaro decided to publish in volumes two and nine of the National Edition:

MS 71 39r	*De Motu Accelerato*	EN2:259–66
MS 71 132v–125r	Latin transl. of Greek Isocrates	EN9:283–4

The last item here is bound in backwards, which explains its folio ordering.

Favaro's arrangement in EN1 suggests that, of the three main items, the twenty-three-chapter treatise on motion was written first, followed by the ten-chapter treatise, and the dialogue on motion last. To these, he inserted the variants between the first two items and appended the memoranda on motion and the plan at the end.

This ordering has been contested in all recent scholarship, starting with Drabkin and Drake (1960) and Fredette (1972). Both proposed the order of dialogue, twenty-three-chapter version, and then ten-chapter version, though they offered different reasons in its support.

To these, Drake (1986) added the evidence he was able to glean from watermarks and on that basis made further decisions regarding the time and place of their composition. In his view, the dialogue was written first, at Siena, between 1586 and 1587; then came the ten-chapter treatise, composed at Florence in 1588, and finally the twenty-three-chapter treatise, also at Pisa, between 1590 and 1591. His dating of the last item was based on my dating of the logical questions (Manuscript 27), whose influence he could also detect in the longer *De Motu*.

In my response to Drake's proposal, I agreed that the dialogue was written first, but at Pisa and in 1590, and I maintained that the

other versions were composed there also, but in 1591 or 1592, before Galileo left for Padua (Wallace 1990:42–7).

This is the way things stood before Camerota began his detailed study of watermarks in Manuscript 71 and Hooper examined its various components for peculiar spellings of Latin terms. Their most important finding was that the ten-chapter *De Motu* was written on paper with the same Florentine watermarks as that of Galileo's lectures on Dante's *Inferno* (and Manuscript 46's treatise on the elements) and had many irregularities in spelling, suggesting that it was the first item of those preserved in Manuscript 71, written in 1588 or shortly thereafter. Of the remaining pieces, all but the last four chapters of the twenty-three-chapter treatise bear Pisan watermarks. These chapters, surprisingly, are written on sheets with Florentine watermarks. The ensemble shows very few peculiar spellings, with the exception of the variants of the first two chapters, which have more than half the percentage of irregular spellings in the ten-chapter treatise and are probably of early composition also.

Data such as these have led Hooper (using Camerota's data) to propose the following as the preferred order of the materials in Manuscript 71: the ten-chapter treatise, composed at Florence as early as 1588, the variants on the first two chapters, written at Pisa in 1590, the *Dialogus*, written at Pisa also in 1590, the first nineteen chapters of the twenty-three-chapter *De Motu*, likewise written at Pisa but in 1591–1592, and the last four chapters of that work, written at Florence in 1591–1592 (Hooper 1993, Camerota 1993).

As supporting evidence for their Pisa 1590 dating of the dialogue, Hooper-Camerota detect the influence of Mazzoni in that work. These results are in substantial agreement with my own datings (Wallace 1990, 1992b). The most important consideration is that the latest research confirms my line of reasoning to establish that the major part of the *De Motu Antiquiora*, and particularly the twenty-three-chapter version, was written after the composition of Manuscripts 27 and 46 (Wallace 1984a). This allows for an influence of the materials Galileo appropriated from the Jesuits on that work, with consequences I shall now explain.

The key teaching of Manuscript 27, already noted, is that on the demonstrative *regressus*, a type of reasoning that employs two demonstrations, one "of the fact" and the other "of the reasoned fact" (Galilei 1988, Berti 1991, Wallace 1992b:180–184). Galileo refers to

these demonstrations as "progressions" and notes that they are separated by an intermediate stage.

The first progression argues from effect to cause and the second goes in the reverse direction, thus "regressing" from cause to effect. For the process to work, the demonstration of the fact must come first, and the effect must initially be more known than the cause, though in the end the two must be seen as convertible. The intermediate stage effects the transition to the second demonstration.

As explained in Galileo's time, this stage involved "a mental examination of the cause proposed," *mentale ipsius causae examen*, the wording used by Jacopo Zabarella.[7] The Latin *examen* is significant because it corresponds to the Greek *peira*, a term that is the root for the Latin *periculum*, meaning test, the equivalent of *experimentum* or experiment (Olivieri 1978:164–6, Wallace 1993). Thus the main task of the intermediate stage is one of testing, of investigating and eliminating other possibilities, and so seeing the cause as required wherever the effect is present.

Note here Galileo's major innovation in the *regressus*: It was his use of the *periculum* in the intermediate stage to determine the "true cause" of the phenomenon under study. In the case of the *De Motu Antiquiora* that phenomenon was the speed of a body's fall in various media. Here Galileo's major use of Archimedes was his replacement of Aristotle's concept of absolute weight by that of specific weight, that is, the weight of the body as affected by the medium in which it is immersed, and so corrected for the buoyancy effect of the medium.

This was Benedetti's contribution, of course, and is not original with Galileo. What was original was Galileo's use of the inclined plane to slow the descent of bodies under the influence of gravity. The basic insight behind this experiment is found in Chapter 14 of the twenty-three-chapter version of *De Motu* (EN1:296–302, Drabkin and Drake 1960:63–9) and may be stated as follows: If the effective weight of a body can be decreased by positioning it on an incline (analogous in some way to the decrease of effective weight by buoyancy), then its velocity down the incline will be slowed proportionately.

The demonstration Galileo offers is geometrical and consists in showing that the forces involved with weights on an inclined plane actually obey the law of the balance. It also invokes several suppositions and on this account may be seen as a demonstration *ex*

suppositione. If these suppositions are granted, the conclusion follows directly: The ratio of speeds down the incline will be as the length of the incline to its vertical height, because the weight of the body varies precisely in that proportion.

Galileo uses the term *periculum* for test or experiment five times in the *De Motu* treatises (Schmitt 1981:VIII, 114–23). One occurrence is in connection with the basic supposition behind his reasoning, the Aristotelian principle that speed of fall is directly proportional to the falling body's weight, amended now to be its weight in the medium as opposed to its absolute weight. Galileo says that if one performs the *periculum* the proposed proportionality will not actually be observed, and he attributes the discrepancy to "accidental causes" (EN1:273).

Moreover, for the inclined plane reasoning to apply, one must suppose that there is no accidental resistance occasioned by the roughness of the moving body or of the plane or by the shape of the body; that the plane is, so to speak, incorporeal, or at least that it is very carefully smoothed and perfectly hard; and that the moving body is perfectly smooth and of a perfectly spherical shape (EN1:298–9). Under such conditions, one may suppose that any given body can be moved on a plane parallel to the horizon by a force smaller than any given force (EN1:299–300). Here Galileo states that one should not be surprised if a *periculum* does not verify this for two reasons: External impediments prevent it (which elicits the previous supposition) and a plane surface cannot be parallel to the horizon because the Earth's surface is spherical (EN1:301).

A more interesting *periculum* to which Galileo makes reference occurs in Chapter 22 of the *De Motu*, where he speaks of dropping objects from a high tower (EN1:333–7, Drabkin and Drake 1960:106–10). Here, he contests the results of Borro's *experimentum* which purported to show that when two equal bodies of lead and wood are thrown simultaneously from a window, the lighter body invariably reaches the ground before the heavier one.

Galileo's tests, which he says were often repeated, show the opposite. Although the lighter body moves more swiftly at the beginning of its motion, the heavier one quickly overtakes it and reaches the ground far ahead. The reasons Galileo offers is that the lighter body cannot conserve its upward impetus as well as the heavier body. Thus it falls quickly at first, but the heavier body then overcomes its

upward impetus and so catches up with, and then passes, the lighter body.

This solution actually depends on Galileo's argument in Chapter 19 of *De Motu*, directed against Aristotle, to explain why bodies increase their speed, or accelerate, during fall (EN1:315–23, Drabkin and Drake 1960:85–94). There, Galileo bases his explanation on an upwardly directed impetus or levity impressed on the body that is self-expending with time. As opposed to Aristotle's cause, Galileo sees the *vera causa* of the velocity increase to lie in the decrease of effective weight throughout the body's fall.

All of these suppositional demonstrations, we now know, pertain to Galileo's Pisan period. They all can be put in the form of the demonstrative *regressus* as this is set out in Manuscript 27, samples of which are given in Wallace (1992a:241–7). Galileo wanted to publish the treatise on motion, but he clearly had doubts about the "true causes" he had proposed in it because of his failure to obtain experimental confirmation of his results. He kept the manuscript in his possession, nonetheless, and when he finally did discover the correct law of falling bodies, he inserted a draft of his discovery among the folios of Manuscript 71, thus signaling its role in the discovery process (Fredette 1972, Camerota 1992). This is the *De Motu Accelerato* fragment we have listed above, which Favaro correctly judged was composed in 1609, at the end of Galileo's early period, and so he published it in the second volume of the National Edition.

CONTINUATION AT PADUA, AND BEYOND

We move now to the next period of experimental and observational activity, this time at Padua and extending to 1610, at the end of which Galileo made his important discoveries with the telescope. In his teaching at Padua, he continued to use his treatise on the sphere, the *Trattato della Sfera*, also called the *Cosmografia*, which is significant for its showing how the demonstrative regress works in astronomy.

The simplest context is Galileo's explanation of the aspects and phases of the Moon and the ways these vary with the Moon's synoptic and sidereal periods (EN2:251–3). These phenomena depend only on relative positions within the Earth–Moon and Earth–Sun systems and do not require commitment to either geocentrism or heliocentrism, being equally well explained in either. Basic to the

explanation is the conviction that these aspects and phases are effects (*effetti*) for which it is possible to assign the cause (*la causa*, EN2:250). Among the causes Galileo enumerates are that the Moon is spherical in shape, that it is not luminous by nature but receives its light from the Sun, and that the orientation of the two with respect to Earth is what causes the various aspects and the places and times of their appearances.

The argument is typically that of a *scientia media* and follows closely the paradigm provided by Aristotle in *Posterior Analytics* (Bk. 1, Ch. 13) to show that the Moon is a sphere. It involves only one supposition, that light travels in straight lines, and this is what governs the intermediate stage of the regress. It allows one to use projective geometry to establish the convertibility condition, namely that only external illumination falling on a shape that is spherical will cause the Moon to exhibit the phases it does at precise positions and times observable from the Earth. The reasoning is summarized in regress form in Wallace (1992a:194–7).

Galileo's first attempt at a science of mechanics followed soon after his *De Motu Antiquiora* and built on the progress he had made at Pisa in the study of the inclined plane. The earliest version of his mechanics, based on what was thought at the time to be Aristotle's *Quaestiones mechanicae*, survives in two early versions, one probably dating from 1593 and the other certainly from 1594.

The main point is to show how all the primary machines – the lever, the capstan, the pulley, the screw, and the wedge – can be reduced to the simplest of them – the lever – and this itself can be reduced to the balance. In it, Galileo uses a concept he had already mentioned in the *De Motu*, namely, that of a minimum force, or a force smaller than any given force, to prove that a force of 200 will move a weight of 2,000 if applied with a leverage of 10 times the distance of application. If one considers, he says, that any minimal moment added to the counterbalancing force will produce a displacement, by not taking account of this "insensible moment," one can say that motion will be produced by the same force as sustains the weight at rest.

The use here of what is clearly a supposition, one permitting the mathematical physicist to neglect insensible forces in his calculations, opened the door for him to treat both dynamic and static cases by the same mathematical principles. Thus, by this early date, he had begun to bridge the gap between Archimedean statics and the Aristotelian dynamical tradition of *De ponderibus* recently revived

by Tartaglia, and he was moving in the direction of a unified science of statics and dynamics.

Galileo's more fully developed treatise on mechanics, written in Italian and titled *Le meccaniche* in some manuscripts, was completed by 1600 or 1602 and was modeled on Tartaglia's works. In it, Galileo attacked the difficult problem of the force required to move an object up an inclined plane. By invoking his principle that the force required to move a weight need only *insensibilmente* exceed the force require to sustain it, he was able to solve not only the problem of the inclined plane but that of the wedge and the screw also (EN2:183–4, Drabkin and Drake 1960:175–7). Again, this line of reasoning made use of the demonstrative regress, invoking in the intermediate stage suppositions of the type described above (Wallace 1992a:262–3).

Shortly after this, Galileo engaged in an extensive period of experimentation that is recorded in the folios uncovered by Drake and that enabled him finally to obtain empirical confirmation of his calculations for motion down an incline and in free fall. This required him to relinquish the Archimedean–Aristotelian ratios for velocity versus specific weight he had been employing at Pisa and, ultimately, by 1609, to arrive at the conclusion that in motions that are naturally accelerated the velocity increases uniformly with time of fall. The major steps in this program, which involved the so-called table top experiments (completely unknown before Drake's discoveries) employed demonstrations that can be arranged in the format of the *regressus*, as will be documented below.

Momentous as these investigations were, they were quickly surpassed by Galileo's discoveries with the telescope in late 1609 and 1610. Fortunately, the paradigm he had used for demonstrating the aspects and phases of the Moon was at hand for explaining the novelties he had revealed. Others before him had constructed telescopes, and some had even looked at the heavens with them, but none would formulate the "necessary demonstrations" Galileo would propose on the basis of his observations.

Within months, he established that there were mountains on the Moon, that Jupiter was carrying along four satellites in its twelve-year passage across the heavens, and, later, that Venus exhibited phases – a sure indication it was orbiting the Sun and not the Earth. So spectacular were these results, all of which could be shown to be demonstrations through the use of the *regressus*, that they changed

Galileo's life in a most profound way. His "early period" was completed and he set out on the fateful course of convincing his fellow scientists (and the Church) that the Copernican system actually portrayed the true construction of the world. This would not only occupy his "middle period," but it would determine the tragic course of his "later period" as well.

When we add these Paduan accomplishments to their Pisan beginnings, however, we can see how fruitful these times leading to Galileo's forty-fifth year had been. His spectacular results in astronomy, no more important than his laying the foundations of modern mechanics, as yet unknown to the world, had behind them the strong logical base contained in Manuscript 27, one of his first Pisan manuscripts. Precisely how he accomplished this is documented in Wallace (1984a, 1992a), the first providing textual selections that connect Manuscript 27 with the various discoveries, and the second showing how all employ a search for causes using a method of resolution and composition that fits into the general schema for the demonstrative regress. The results are tabulated below, with the subjects of proof indicated in the center, the page numbers in 1984a on the left, and those in 1992a on the right:

Text (1984a)	Subject of proof	Manuscript 27 (1992a)
230	1 Fall in Various Media (EN1)	242
233	2 Fall and Specific Weight (EN1)	248
236	3 Speed in Different Media (EN1)	250
239	4 Motion on Inclined Planes (EN1)	253
235	5 Speed Increase in Fall (EN1)	256
248	7 Aspects and Phases of the Moon (EN2)	195
—	8 Mountains on the Moon (EN3.1)	199
—	9 Satellites of Jupiter (EN3.1)	202
—	10 Phases of Venus (EN10)	202

Of these, the first five, all from the *De Motu Antiquiora*, were not strictly demonstrations, although Galileo originally proposed them as such. It surely is to his credit that he ultimately recognized this and withheld them from publication, undoubtedly for empirical reasons, because of their failure to meet the limited *pericula* he used to test them at Pisa. Of the remainder, and particularly the last four, he never doubted their apodictic character.

There remains now a final consideration, namely, whether Galileo's use of demonstrative techniques terminated in 1610 at the end of his early period or whether it extended into the other periods as well. There are excellent reasons to prefer the second alternative, especially when one sees Galileo as amending the Manuscript 27 doctrine to make of it a logic of discovery that can employ probable arguments as well as demonstrative proofs.

The first indication we see of this is his tentative proof for the Earth's motion based on the ebb and flow of the tides, which he presented to his friend Cardinal Alessandro Orsini on January 8, 1616 (EN5:377–95). There, Galileo speculates that "the cause of the tides could reside in some motion of the basins containing the seawater," thus focusing on the motion of the terrestrial globe as "more probable" than any other cause previously assigned (EN5:381). In concluding his proof, Galileo notes that he is able to harmonize the Earth's motion with the tides, "taking the former as the cause of the latter, and the latter as a sign of and an argument for the former" (EN5:393). This is an elegant way of reformulating the first and last stages of the demonstrative regress, while leaving the intermediate stage open for probable arguments as well as for those that would establish conclusive proof.

Using this enlarged understanding of the *regressus*, it is possible to analyze the key proofs Galileo worked out in his middle and later periods. These are presented below in a format similar to that used above for the early period:

Text (1984a)	Subject of proof	Manuscript 27 (1992a)
284	1 True Cause of Flotations (EN4)	277
288	2 Nature of Sunspots (EN5)	209
294	3 Early Tidal Argument (EN5)	212
300	4 Unity of the Universe (EN7)	220
303	5 Earth's Daily Rotation (EN7)	223
306	6 Earth's Annual Revolution (EN7)	225
308	7 Later Tidal Argument (EN7)	229
315	8 True Cause of Cohesion (EN8)	281
320	9 Breaking Strength of a Beam (EN8)	283
322	10 Naturally Accelerated Motion (EN8)	287
330	11 Motion of Projectiles (EN8)	292

The first of these, as well as the eighth to eleventh, Galileo seems to have proposed as demonstrative. The rest he proposed only as probable arguments, surely because of the Church's prohibition against Copernican teaching, but also because he may have recognized some of their logical limitations. By the time he came to the last two, however, there can be no doubt that he made the transition from *scientia media* to *nuova scienza* (Olivieri 1995) and it is for this we celebrate him as the Father of Modern Science.

Only sixteen months before his death, on September 14, 1640, Galileo wrote a letter to Fortunio Leceti, explaining what it meant to be a true follower of Aristotle and stating that, in matters of logic, he had been an Aristotelian all his life (EN18:248). In light of his many invectives against the Peripatetics of his day, this statement by Galileo is puzzling and has given rise to many interpretations, some calling into question his honesty and sincerity.

When the letter is read in light of the materials just presented, however, it is a simple matter to absolve Galileo of charges of this type. In effect, he does not commit himself to any of Aristotle's conclusions in the physical sciences but states instead that he has consistently followed Aristotle's logical methodology in his own scientific work.

This is what enabled him, he says, to reason well and to deduce necessary conclusions from his premises; coupled with what he has learned from pure mathematicians, it has given him skill in demonstration and the ability to avoid mistakes in argumentation. He concludes on the note that, if one takes reliance on Aristotle's logical canons to be the sign of a Peripatetic, he can rightfully be called a Peripatetic himself.

When the letter to Liceti is read in light of what is available in the National Edition alone, of course, the background required for its understanding is missing. But then the true problem posed by the letter becomes quite clear: It is not Galileo's identifying himself as an Aristotelian but rather how he could possess sufficient knowledge of Aristotelian logic to be able to employ it in the way he claims. The problem is insoluble when the manuscripts of his early period, and particularly his Manuscript 27, are overlooked or are not taken into account. Such omission is the *parvus error in initio* to which I referred at the outset of this essay. Only when it is rectified do we gain an understanding of the man within his full historical context.

NOTES

1 *Le Opere di Galileo Galilei*, ed. Antonio Favaro, 20 vols. in 21, Florence: G. Barbèra Editrice, 1890–1909, henceforth cited as EN: Vol. No., page no(s).
2 Following the lead of W. R. Shea (1972), the chronology of Galileo's life is now commonly divided into three periods – the early period, from his birth in 1564 to 1610; the middle period, from 1610 to 1632; and the later period, from 1632 to his death in 1642.
3 The pioneering study of the sources of Galileo's natural philosophy was that of Alistair Crombie (1975), who first discerned its connection with teachings of the Jesuits. He followed that essay with a study of the place of mathematics and Platonism in Jesuit educational policy (1977) and then with a fuller examination of Jesuit ideas of science and of nature that are reflected in Galileo's writings, which he coauthored with his student Adriano Carugo (1983). For a detailed account of my early investigations and their relationships to the work of Crombie and Carugo, see Wallace (1984b:xii–xiii, 1986b, 1986c, and 1992b:xi–xv).
4 Here, Hooper reported early results of a project at the Instituto Nazionale di Fisica Nucleare in Florence, in which accurate physical analyses are being made of the chemical composition of Galileo's inks and papers using nondestructive proton induced x-ray emissions (acronym PIXE).
5 Favaro dated the compositions of Manuscript 46 at 1584, on the basis of the internal evidence he gathered from that chronology (EN1:27), where he added the number of years Galileo gives "from the birth of Christ to the destruction of Jerusalem, 74; from then up to the present time, 1510," to get the result 1584. Apparently, Favaro was unaware that exegetes in Galileo's time had already established that Christ was born in the year 4 B.C., and thus he should have obtained the result 1580. What he also could have done was add A.D. 70 (a well-established date among historians for the destruction of Jerusalem) to 1510, and this would have given him 1580 directly. If Galileo used Menu's notes for the chronology, this would serve to explain the sum of 1580 in his appropriation. Part of Favaro's reason for defending his erroneous 1584 dating of Manuscript 46 seems to have been his opposition to Pierre Duhem, who used Galileo's mention of the *Doctores Parisienses* in that manuscript to connect him with medieval authors who were his so-called Parisian precursors. For details of the dispute between Favaro and Duhem, see Wallace (1978) which is enlarged and reprinted in Wallace (1981).
6 Works on the authors and subjects mentioned and on which I have drawn in what follows include Camerota (1989), DePace (1990, 1992), Lennox (1986), Machamer (1978), Manno (1987), Masotti (1976), and Schmitt (1981).
7 The expression occurs in Zabarella's *Opera logica*, Cologne: Zetzner, 1597, 486. For details of the connection between Galileo and Zabarella, see Wallace (1988), reprinted in Wallace (1991).

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XII

Circularity and the Paduan Regressus: *From Pietro d'Abano to Galileo Galilei*

The problem of whether or not Galileo Galilei was influenced by the Paduan Aristotelians in his adoption of a scientific methodology has been the subject of considerable controversy, beginning with Ernst Cassirer in the first volume of his *Das Erkenntnisproblem in der Philosophie und Wissenschaft der neueren Zeit* (Berlin 1922) and continuing to the present day. In 1940 John Herman Randall, Jr., made a strong case for such an influence on the basis of Galileo's use of the demonstrative *regressus*,[1] but recent scholars have tended to overlook his work. However, with the transcription and publication in 1988 of Galileo's *Tractatio de demonstratione*,[2] written four hundred years earlier, in 1588 or 1589, the situation has begun to change. This *Tractatio*, which unfortunately was left out of the National Edition of Galileo's works, contains a complete description of the *regressus*. There is substantial evidence that Galileo not only knew the details of the *regressus* but employed it to found the "new sciences" for which he is famous. But this discovery is now prompting another *Methodenstreit*, this one questioning the validity of the *regressus* on the grounds of its inadequacy and thus suggesting that, even had Galileo used it as a method, it could not have gotten the results he claimed for it.[3] The time seems ripe to

[1] *The Development of Scientific Method in the School of Padua*, in: Journal of the History of Ideas, 1 (1940), 177-206. This has been reprinted in *The School of Padua and the Emergence of Modern Science*, Padua 1961, 16-68. Randall himself has surveyed reactions to his thesis in his *Paduan Aristotelianism Reconsidered*, in: E.P. Mahoney (ed.), *Philosophy and Humanism: Renaissance Essays in Honor of Paul Oskar Kristeller*, New York 1976, 275-82.

[2] The full title is Galileo Galilei, *Tractatio de praecognitionibus et praecognitis* and *Tractatio de demonstratione*, eds. W.F. Edwards and W.A. Wallace, Padua, 1988. The Latin text of the teaching this contains on the demonstrative regress is reproduced in my *Randall* Redivivus: *Galileo and the Paduan Aristotelians*, in: Journal of the History of Ideas, 48 (1988), 133-49. *

[3] The beginnings of the controversy, which also centers on elements of continuity between late medieval and early modern science, are found in Nicholas Jardine, *Galileo's Road to Truth and the Demonstrative Regress*, in: Studies in History and Philoso-

reopen the older controversy and see what it may have to contribute to the resolution of the new one.

Foundations in Aristotle and Galen

A peculiar thing about the *regressus* is that the term itself is Latin and has no direct counterpart in Greek; thus it is not found in Aristotle's text.[4] Yet the doctrine is clearly Aristotelian in origin, though it did not assume identifiable form as related to a scientific methodology until the second century, when it was discussed by Galen in his *Ars medica, quae et ars parva dicitur* (I,4). Then it was taken up by Greek commentators on Aristotle in the fourth century, following Themistius, and finally it received its fullest treatment from Averroes in a number of his writings.[5] In view of this development, the simplest way to locate the foundations of the teaching in Aristotle is to identify the texts in his corpus that are most frequently cited by these early authors and then examined in detail by the Paduan Aristotelians. These are fairly numerous, but the main sources are the following: *Physics* I,1; *Prior Analytics* II,5,8,20; *Posterior Analytics* I,2,3,13,22; *Topics* II,2; *Nicomachean Ethics* I,2,3; and *Metaphysics* V,11.[6]

Posterior Analytics I,13 provides the best general idea of what the *regressus* is all about when it introduces the distinction between a dem-

phy of Science, 7 (1976), 277-318. Additional background will be found in Ernan McMullin, *The Conception of Science in Galileo's Work*, in: R.E. Butts and J.C. Pitt (eds.), *New Perspectives on Galileo*, Dordrecht-Boston, 1978, 209-57; and Winifred L. Wisan, *Galileo's Scientific Method: A Reexamination*, in: ibid., 1-57. See also McMullin's review of my *Prelude to Galileo* (note 34, below) in: Philosophy of Science, 50 (1983), 171-3, and my reply to him in my *Galileo and the Continuity Thesis*, in: Philosophy of Science, 51 (1984), 504-10. More recently Zev Bechler, in his *Newton's Physics and the Conceptual Structure of the Scientific Revolution*, Dordrecht-Boston-London 1991, has attacked Newton's method of resolution and composition as involving circular reasoning, and extends the same critique to Galileo, 105-71.

[4] Although there is no term for *regressus* in Greek, there is another term that comes close to it, namely, circle (*kuklos*), in the sense of circular reasoning. Aristotle makes frequent reference to that type of reasoning in a variety of contexts.

[5] The texts cited by Themistius are found in his *Analyticorum Posteriorum paraphrasis* I,2-3,20-22 and his *In libros De anima paraphrasis* I,7-9. Those cited by Averroes are in his *Posteriorum Resolutoriorum libri duo. Expositio magna*, I, text. comm. 95-102; *Epitome in primum librum Posteriorum*, 2-4; *De Physico auditu libri octo... Commentaria in eosdem magna*, I, text. comm. 2-5; and *Expositio media super libros Physicorum*, I,2-5.

[6] Here and in what follows I have drawn freely on the more detailed account by Giovanni Papuli, *La teoria del* regressus *come metodo scientifico negli autori della Scuola di Padova*, in: Luigi Olivieri (ed.), *Aristotelismo Veneto e Scienza Moderna*, 2 vols., Padua 1983, Vol. I, 221-77, henceforth cited as Papuli.

onstration "of the fact" (*quia*) and a demonstration "of the reasoned fact" (*propter quid*). One of the illustrations of demonstration *quia* that Aristotle provides is the proof that the moon is a sphere from its having phases; in this case the middle is its having phases and the major is its being spherical in shape. If the middle and the major are convertible, Aristotle goes on, the two terms can be interchanged and then the proof is converted into a demonstration *propter quid.* Whereas the first had merely established the "fact" of the moon's being a sphere, the second uses this fact to provide the reason why it exhibits phases and thus is a demonstration "of the reasoned fact." The line of reasoning progresses from the effect (having phases) to the cause (being a sphere), and then goes back again (or regresses) from this cause to the effect with which the reasoning started. The effect in this case is an appearance grasped in sense perception, and yet it is able to provide universal knowledge of the cause, one on which a *propter quid* demonstration can be based. Toward the end of the chapter Aristotle notes that these two types of demonstration might pertain to the same science, but they can also pertain to different sciences. The examples he cites are from the so-called "mixed sciences," such as optics, which uses geometrical principles to study phenomena like the rainbow.[7] Here, he observes, it is the business of the physicist to know the fact and of the mathematician to know the reasoned fact. In this case the physicist knows the appearances, but the mathematician has an insight into the universal.

The remaining texts now serve to illuminate this teaching in different ways. *Physics* I,1, *Nicomachean Ethics* I,2-3, and *Metaphysics* V,11 are all concerned with the order of human knowing. They state, in

[7] Here again the term "mixed sciences" has no equivalent in Aristotle's Greek text. The expression he uses is sciences that are contained "one beneath the other" (*thateron hupo thateron*). In commenting on the text Zabarella refers to these as "subalternate sciences" (*scientiae subalternatae*), the superior of which is subalternating (*subalternans*) and the inferior subalternated (*subalternata*). Thomas Aquinas called them "middle sciences" (*scientiae mediae*) because they are intermediate between mathematics and physics. Other Latin commentators named them "mixed sciences" (*scientiae mixtae*) because the reasoning they involve uses elements of both. The branch of mathematics that treats such subalternating sciences then came to be known as "mixed mathematics" (*mathematica mixta*). Aristotle himself refers to the corresponding subalternate sciences as "the more physical of the mathematical sciences" (*ta phusikōtera tōn mathematōn*). For details, see Richard D. McKirahan, *Aristotle's Subordinate Sciences*, in: British Journal for the History of Science, 11 (1978), 197-220, and James G. Lennox, *Aristotle, Galileo, and 'Mixed Sciences,'* in: W.A. Wallace (ed.), *Reinterpreting Galileo*, Washington, D.C. 1986, 29-51.

slightly different ways, that we proceed from things that are more known and clearer to us to those that are clearer and more known by nature or in themselves. Usually the idea is that particulars or things of sense are first grasped by us, and then we come to the knowledge of universals or ideas that are more knowable in themselves. The texts from *Prior Analytics* II,5,8,20 are concerned with formal details of the conversion of syllogisms by interchanging middles and with the general problem of circular and reciprocal proof. Those of *Posterior Analytics* I,2-3,13 then go into material details relating to circular and reciprocal vs. non-reciprocal proof. Finally, *Posterior Analytics* II,22 and *Topics* II,2 provide arguments to show that there cannot be an infinite number of middle terms or, alternatively, that the regress of middles must ultimately terminate.

It is noteworthy that the term "method," which comes from a transliteration of the Greek *methodos*, does not appear in the *Posterior Analytics*, Aristotle's basic treatise on scientific methodology, though it is found in his other writings. Derived from *meta*, meaning "after" or "following," and *hodos*, meaning "way," the Greek compound originally was taken to mean the way or order to be followed in rational inquiry. In this meaning it implied the rules or norm according to which inquiry was to be conducted, and in such usage logic was said to be a method. From the idea of norm the term was transferred to a discussion or questioning that proceeded along a logical path, as in the expression "Socratic method." The word then came to mean any doctrine attained as the result of such inquiry, and thus the term *methodoi* was used to designate various schools or philosophies. This usage is found in the medical writer Galen, from whom it passed ultimately into the Paduan tradition.[8]

Galen's ideas were basically Aristotelian, but they also drew much from his medical predecessor Hippocrates and from the works of Plato, particularly the *Phaedrus*. In this dialogue Socrates points out the similarities between the work of the physician and that of the dialectician: the physician must know the nature of the body and the dialectician the nature of the soul, and to know either requires the *methodos* used by Hippocrates. What this method involved was a composite of different procedures: the analytical, the definitive, the

[8] Details are given in J.A. Weisheipl, *The Evolution of Scientific Method*, in his *Nature and Motion in the Middle Ages*, ed. W.E. Carroll, Washington, D.C. 1985, and in Neil W. Gilbert, *Renaissance Concepts of Method*, New York and London 1960.

divisive, and the demonstrative. Within this setting, *methodos* was always associated with art or *technē*. Aristotle's contribution was to extend the concept to all types of rational inquiry, including the sciences, and point to analysis and definition as its primary components.

Galen himself wrote a major work on method. This was lost sometime after the sixth century, and various attempts to reconstruct it are based on references to methodology in his other writings. From these we can gather that he focused first on analysis, then on synthesis; in association with these he also spoke of definition and division. In all ways of teaching (*doctrinae*), he stated, there are three orders of procedure. One follows the way of conversion and resolution (*dissolutio*), in which the investigator takes the object of scientific knowledge as the end to be aimed at, then seeks what lies nearest to it and without which the thing cannot exist, then what precedes that, and so on, until a principle is arrived at. The second way follows the way of composition, and is the contrary of the first. This begins with the principle and composes these (*compone eas*), that is, the elements arrived at, in their inverse order until the object is again reached. Then the third way follows the way of analyzing the definition. Galen's references to *methodos* are usually in the plural, as when he writes of logical methods and scientific methods, but sometimes they are in the singular, as when he mentions the demonstrative method (*apodeiktikē methodos*).[9]

Paduan Beginnings

A long interval separates Galen from the first Paduan to elaborate on his teachings, Pietro d'Abano (1257-1315). Although methods of resolution and composition were used earlier in the Middle Ages—by Robert Grosseteste at Oxford and by Albertus Magnus and Thomas Aquinas at Paris—interest at both those universities was focused primarily on speculative disciplines such as metaphysics and theology. The strong medical orientation at Padua was perhaps instrumental in directing attention there to medicine, logic, and the natural sciences. In any event, it was in this setting that Abano combined the teachings

[9] The text on which this analysis is based is cited and translated from the *Galeni principis medicorum Microtegni cum commento Hali*, n.d., by Randall in *School of Padua*, 31-2. It is also translated in my *Causality and Scientific Explanation*, Vol. 1, Ann Arbor 1972, 120. This will be cited as *Causality* in what follows; along with Papuli, *Causality* and *School of Padua* provide most of the documentation needed for this essay.

of Aristotle's *Posterior Analytics* with those of Galen's *Ars medica.* His objective was to reconcile the teachings of the philosophers and the medical doctors, as in the title of his major work, *Conciliator differentiarum philosophorum, et praecipue medicorum.* This was written in 1310 and then published at Venice in 1476; it earned for him the title "the Conciliator," by which he is generally known.

For Abano science in the proper sense infers conclusions from causes that are proximate and immediate, and this is what Aristotle in the *Posterior Analytics* called *demonstratio propter quid* and Galen, *doctrina compositiva.* There is another sense of science that is also proper, he goes on. Indeed, this is most proper because it is best adapted to human modes of knowing, since the natural way for us is to proceed from what is more knowable and certain for us to what is more knowable in the order of nature. Aristotle describes that way at the beginning of the *Physics*; it is what he identifies elsewhere as *demonstratio quia*, and Galen, *doctrina resolutiva.* In it we proceed in the opposite order from effect to cause through proximate and logically immediate middle terms. Another type of *demonstratio quia* is had when we conclude an effect from more general causes, omitting specific causes that are in between.[10]

The method here outlined by Abano is somewhat similar to that contained in the writings of Averroes already mentioned in note 5. Perhaps on this account Abano is said to have introduced Latin Averroism from Paris, where he had studied, to the University of Padua. However that might be, an Averroist commentary on the *Physics* written in 1334 and published at Venice in 1492 pointed to the three types of demonstration identified by Averroes. In this context it notes that causes such as are found in natural science, though prior and more known in the order of nature, are often posterior and less known to us. Such causes we investigate through effects that are prior for us, and this is the method of resolution. Then, after we have discovered the causes, we demonstrate the effects through them, and this is the method of composition.[11]

[10] *Conciliator*, ed. Venice 1496, Diff. 3, prop. 1; *School of Padua*, 28-9, *Causality*, 119. Later on in the *Conciliator* Abano cites the text from Galen's *Microtegni* (known in Latin as the *Ars medica, quae et ars parva dicitur*) referred to in the previous note. See also Papuli, 225-9, 274.

[11] The text is that of *Urbanus Averoysta philosophus summus...commentorum omnium Averoys super librum Aristotelis de physico auditu expositor*, Venice 1492, comm. text. 2, cited and translated in *School of Padua*, 39-40; see *Causality*, 121.

The most important thinker at Padua in the century that followed was the Augustinian friar Paul of Venice (1369?-1428), who had studied at Oxford and Paris before teaching at Padua. In his commentary on the *Physics* Paul cites Averroes as recognizing a double procedure in natural science, one from effect to cause, the other from cause to effect. He explains that in physics one begins both from causes and from what is caused, though in different ways. One way is from causes understood inclusively (*inclusive*), by knowing them, the other from effects understood exclusively (*exclusive*), by knowing by means of them. Thus there is a twofold knowledge of the cause, one obtained by a procedure *quia*, the other by a procedure *propter quid*; the second kind depends on the first, and the first is the cause of the second. Then, in his commentary on the *Posterior Analytics*, Paul uses the notion of necessary connection to explain how a demonstration *quia* can sometimes be converted to a demonstration *propter quid.* This can only be done if there is a "mutual necessary relationship" between cause and effect such that, if the cause is placed the effect is placed also, and vice versa. The example he cites is an eclipse and the interposal of the earth between the sun and the moon. In his *Summa naturalis philosophiae*, finally, Paul takes up the objection that the use of this double procedure involves one in circular reasoning. His reply is that knowledge of an effect through *propter quid* reasoning is not the same as knowledge of an effect through reasoning *quia*; in the second case the effect is known in itself, in the first through what causes it. There would be circularity if the knowledge were of the same kind, whereas in this case the knowledges are different. But in Paul's expositions, although the *ordo mentalis* always precedes the *ordo naturalis*, his emphasis is on the connectedness between the two, a point that Averroist commentaries tended to overlook. They focused on Averroes's three types of demonstration, *demonstratio signi, demonstratio causae*, and *demonstratio simpliciter*, generally seeing the three as unrelated procedures.[12]

A similar line of thought is found in Paul's contemporary, Ugo Benzi of Siena (d. 1439), who insists that any *doctrina* must involve a setting forth of what is demonstrable, a *manifestatio demonstrabilis.* As practiced in physics and medicine, he states, this involves a double process, the first of which begins with the effects and seeks their cause and the second explains those effects through the cause newly

[12] *School of Padua*, 30-1; *Causality*, 121-7; cf. Papuli, 229-30, 274.

discovered. The process of discovery (*inventio*) is resolutive, whereas that of setting forth the consequences (*notificatio*) is compositive. But Ugo denies scientific fertility to the second process, finding this only in the resolutive method that discovers the middle term.[13]

A similar observation, expressed more forcefully and with a certain intransigence, is found in the teachings on the *regressus* advanced by
* Francesco Securo di Nardo (fl. 1480), usually referred to as Neritonensis. A Dominican philosopher and professor in the Thomistic chair at the University of Padua, he taught Tommaso de Vio Gaetano, Gaspar Contarini, and Pietro Pomponazzi. He left nothing in writing, but his views, which show him even more removed from the Averroists, are reported by Pomponazzi and Girolamo Balduino. Basically Neritonensis denies all probative value to the putative part played in the *regressus* by demonstration *propter quid*, regarding it more as a dialectical syllogism, a logical construct, since it is in evident conflict with our human mode of knowing, which should always proceed from the more known. The being of the effect is what produces our knowledge of the being of the cause. It is true that nature acts by causing, but that a determinate cause will produce a determinate effect is only a conjecture, a *suppositio*. If the effect is there the cause must be also, but if the cause is there there is no necessity that the effect follow. As Neritonensis sees it, the *quia* reasoning that precedes the *propter quid* arrives at the existence of the cause, but it tells nothing about the essence of the cause or how the causing of the effect takes place. Thus the reasoning that is regarded as *propter quid* is not truly demonstrative.[14]

Pietro Pomponazzi (1462-1525) reacted against this teaching of his mentor. He concedes to Neritonensis that scientific knowledge begins in the senses, but in the study of nature one cannot remain only with the *sensatum*. The human mind grasps the sensible first through the cogitative power, and then the universal through the power of intellect, actually seeing it in what is sensed. Thus reasoning *propter quid* is not topical but demonstrative; it seeks to know not merely

[13] *Causality*, 126-7; *School of Padua*, 37-8; Papuli, 230-1, 274. Here Papuli's interpretation of Ugo's teaching differs from Randall's. Papuli holds that Ugo did not attach any scientific value to the demonstration *propter quid*, whereas Randall sees him as regarding both the *quia* and the *propter quid* as essential moments in a strictly scientific procedure.

[14] Neritonensis's arguments are stated and refuted in *Hieronymi Balduini Quaesita aliquot et logica et naturalia*, Venice: Joan. Gryphius, 1563, fols. 217v-218v; Papuli, 231-3.

existence, or definition, but *why* the effect takes place. Yet its type of reasoning has to be joined to *quia* reasoning in the *regressus*, and, as a consequence, if the *quia* reasoning has already provided knowledge of a cause that is *per se* and essential, its production of the effect is likewise essential and not merely accidental. In this way of thinking, the strength of the demonstration *quia* is that it serves as a *via resolutiva* when science is in the investigative mode of discovery; the strength of the demonstration *propter quid* is that it serves as a *via compositiva* when science is in the mode of systematizing knowledge already acquired. And when the *regressus* itself is seen as a *demonstratio potissima* uniting both the *quia* and the *propter quid*, then the *ordo mentalis* and the *ordo naturalis* complement each other in the knowing process.[15]

The Developing Tradition at Padua

With this the ground was well prepared for the refinement of teaching on the *regressus*, which by the sixteenth century had come to be the common term for the double process being discussed by these authors, and which served to distinguish it from *circulatio*. Perhaps the fullest account is that found in the writings of Agostino Nifo (1470-1538), who had carefully analyzed the commentaries of Themistius and Averroes, among others. In showing that the *regressus* is not circular he explained that four types of knowledge were then being distinguished at Padua: that of an effect through sense experience; that of a cause discovered through the effect; that of the same cause through the work (*negotiatio*) of the intellect, which fits it to serve as the middle term of a demonstration; and that of the same effect, known now through a *propter quid* demonstration based on that cause. Since the knowledge of the effect in the last type is quite different from that in the first type, there is no circular argument but rather a true regress.

As Nifo explains the term *negotiatio*, this involves composition and division. When the cause has been discovered, the intellect composes and divides until it knows the cause not only as a cause but also as a middle term and a definition. The *negotiatio* is a grasping of the cause as a middle term and definition. But since a definition is reached only by composition and division, it is through them that the cause is seen

[15] Pietro Pomponazzi, *Quaestio de regressu*, in: *Corsi inediti dell'insegnamento padovano*, 2 vols., ed. Antonino Poppi, Padua 1970, Vol. 1, 153-76; Papuli, 233-5, 274.

under the formality (*ratio*) of a middle term, from which one can proceed to a strict demonstration of the effect.[16]

Other major logicians of the sixteenth century reacted variously to Nifo's teachings. Marcantonio Zimara (1475-1537?), the preeminent Averroist of the time, adjusted his understanding of the *regressus* to incorporate the three types of demonstration recognized by Averroes, namely, *quia, propter quid*, and *simpliciter*. For him, the demonstration *quia* of the *via resolutiva* plays only an auxiliary role, since only the *via compositiva* is strictly speaking demonstrative. Yet he identifies the *regressus* itself with demonstration *simpliciter*, since effectively it combines in a unitary process the functions of demonstrations both *quia* and *propter quid.* Zimara was also much interested in methodological questions. He sharpened the distinction between mathematics, which demonstrates *a priori*, and physics, which he sees as demonstrating *a posteriori* even when reasoning *propter quid* from causes to effects. He also differentiated between order (*ordo*), which is concerned with the teaching and exposition of a subject matter, and method (*methodus*), which is concerned with the discovery of principles and the demonstration of properties. In the order of teaching the passage is from the less difficult to the more difficult, which corresponds to that from the more known to the less known; both are instances of going from what is prior with respect to us (*prius nobis*) to what is posterior (*posterius*). Then the contraposition of the *prius nobis* to the *prius naturae*, moving from what is prior with respect to us to what is prior in nature, is precisely that reflected in the processes of resolution and composition. In such a context Zimara offers a novel reinterpretation of Galen, proposing that the *via resolutiva* in the speculative sciences as well as the practical disciplines involves him in a twofold analysis, one *a posteriori ad prius*, the other *a priori ad posterius*.[17]

16 These teachings are contained in Nifo's *Expositio super octo libros de physico auditu*, Venice: Octavianus Scotus, 1508. The texts are cited in *School of Padua*, 42-3; *Causality*, 140-1; cf. Papuli, 235-6, 275. In a later *recognitio* Nifo revised his teaching on the four types of knowledge, withdrawing his explanation of the *negotiatio*, and holding that the other three types are sufficient. He further held that the second process does not result in an absolute demonstration (*demonstratio simpliciter*) but only in a *demonstratio coniecturalis*, since the discovery of the cause is not obvious to us absolutely, but only conjecturally. Thus it appears that he was eventually won over by Neritonensis. For details, see *Causality*, 141-3.

17 Marcantonio Zimara, *In primum Posteriorum*, Biblioteca Ambrosiana, MS D 109 inf., fols. 17-29 (c. 1521), and *Theoremata, seu memorabilium propositionum limitationes*, ed. Naples 1523, props. 14, 61, both referenced by Papuli, 236-8, 275.

Another Averroist, Tiberio Bacilieri (1461?-1511?), devoted himself to an intensive logical investigation of the concepts involved in the *regressus*. He had studied under Alessandro Achillini at Bologna, where Achillini was insisting that the order of teaching or exposition should follow the order of nature. Bacilieri noted that the *ordo doctrinae*, according to Galen, involved a threefold procedure: resolutive, compositive, and definitive. In the *ordo inventionis*, as opposed to this, Bacilieri identified a threefold process, the first of which is *obscurus*, where the starting point is a simple unknown (*ignotum incomplexum*) grasped in sense experience, the second is *difinitivus*, where the unknown is defined, and the third *scientificus*, where the unknown is grasped scientifically. The second and third steps combine elements of the *via resolutiva* and the *via compositiva*. From this type of analysis Bacilieri goes on to deny any scientific validity to the first step of the *regressus*. In his view, only the part offering a demonstration *propter quid* gives knowledge of the real world; the rest is merely a logical construction.[18]

Tommaso de Vio Gaetano (1468-1534), like Neritonensis a Dominican and a Thomist, came to conclusions not very different from those of Bacilieri. He attacked the Averroist position that there are three different species of demonstration, *quia, propter quid*, and *simpliciter*, the last of which was also called "most powerful" (*potissima*). Averroes regarded *demonstratio simpliciter* as a separate species of demonstration; Gaetano defined this type differently from Averroes and saw it merely as a subspecies of *demonstratio propter quid*. The latter, for Gaetano, carries the whole burden of the *regressus*, not *demonstratio quia*, as it had for Neritonensis. The main problem that engaged Gaetano was the convertibility of the middle term in the two demonstrations and precisely how this could be effected.[19]

The high point of this developing tradition at the University of Padua was reached in the works of Girolamo Balduino (fl. 1550), who began teaching at Padua in 1528 and many of whose teachings anticipated those of Jacopo Zabarella, to be treated in the following section. In his various *Quaesita* on problems in logic and natural philosophy, published at Venice in 1563, Balduino summed up the

[18] Tiberio Bacilieri, *Lectura in universam Aristotelis et Averrois Dyalecticam facultatem*, ed. Pavia 1512; referenced by Papuli, 238-40, 275.
[19] Tommaso de Vio Gaetano, *In libros Posteriorum Analyticorum Aristotelis additamenta*, Ed. Venice 1505, fols. 7r-24r; referenced in Papuli, 240-1, 275.

teachings of the Greek commentators, Averroes, and his immediate predecessors and contemporaries, analyzing them in great detail and, on the basis of that analysis, advancing his own positions. His query *An detur regressus demonstrativus* is obviously of most interest for our study. In the edition just cited the first part of this is concerned with the various species of demonstration, but in a later edition, Venice 1569, that becomes a special query, *De speciebus demonstrationis.* Also relevant to our study is his query on the relative importance of definition and demonstration as instruments of scientific knowing, *Utrum est nobilius instrumentum sciendi, definitio an demonstratio.*[20]

In treating the species of demonstration Balduino rejects the teaching of Gaetano and defends instead the position of Averroes and Zimara, namely, that there are three species: *quia*, which proves a cause from its effect; *propter quid*, which proves only the cause of the effect but not the effect's existence; and *potissima*, which proves both the cause and the existence of the effect. Then, in treating the relative superiority of definition vis-à-vis demonstration, Balduino rejects the teachings of Simplicius, Philoponus, and most of the Latin commentators that demonstration is the most important. Instead he again supports the position of Averroes, holding that definition is superior. These are indications that Balduino should be situated generally among the Paduan Averroists. When he comes, therefore, to taking a position on the *regressus*, after having discussed in detail all previous positions—those of the ancient Greeks (against whom Aristotle argued), the later Greeks (Philoponus, Alexander Aphrodisias, and Themistius), Avicenna, Ugo Benzi, Neritonensis, and various unnamed contemporaries (*moderni*)—it is not surprising that what he takes as the true position is identified with that "of Aristotle and Averroes."[21]

With regard to the *regressus* itself, Balduino defines this as "an artificial process that is scientific, a mutual showing (*monstratio*) of the effect through the cause by the conversion of the middle term with the

[20] We have used the Venice 1563 edition of Balduino's *Quaesita logica et naturalia*, which contains nine queries on logic and two on natural philosophy. Of the nine queries on logic the three that are directly pertinent to this study are *An detur regressus demonstrativus*, fols. 216r-225v; *De medio demonstrationis potissimae*, fols. 226r-235r; and *Utrum est nobilius instrumentum sciendi, definitio an demonstratio*, fols. 235v-237v.

[21] *Quaesita aliquot*, fol. 223v. His previous exposition of various positions is treated in two chapters, the second explaining the teachings of Aristotle and Averroes, fols. 221v-223v, and the first the teachings of all the other commentators, fols. 216v-221v.

major, not in the manner of demonstration and not seeking the same goal."[22] By "artificial process" here Balduino means that it uses the art of logic, not an otiose reasoning of the type "A is because A is" (equivalent to the modern "if p, then p"); rather it makes use of demonstrations, both *quia* and *propter quid.* Since both of these are productive of science, and their end is science, the process is also "scientific." The point of the expression, "a mutual showing of the cause through the effect, and vice versa," is to show how the end of science is achieved. For, since we principally intend to know the "why" (i.e, the *propter quid*) of the effect, and this cannot be had except from its cause, and the cause is unknown, it is necessary first to demonstrate the existence of the cause from the existence of the effect. This requires the "mutual showing" of the cause first through the effect, and then of the effect through the cause by the "proper conversion" of the necessary middle term. In the first demonstration the middle term does not "show" in the same way and to the same end as it does in the second. This Balduino explains as follows: if the effect "shows" the cause in the first *processus*, in the second the cause does not show the effect to exist, nor are cause and effect sought under the same aspect, namely, that of existence, for in the second it is the "why" of the effect's existence that is sought. From this, he goes on, it can be seen that the *regressus* is not a circle, for a circle would return to the point from which it started. Rather, it moves in the form of a triangle: from the effect's existence to the cause's existence; then from the cause's existence to the "why" of the effect's existence. If the second step were back to the effect's existence, rather than to the "why" of that existence, the argument would be circular. In the *regressus* clearly it is not.

The principal end or goal of the *regressus*, as Balduino sees it, is to have perfect and certain science of an effect through its proper cause. This is the procedure Aristotle followed in the first book of the *Physics* in finding the first principles of natural things and also, as Averroes points out, in the eighth book of the *Physics*, in showing the existence of the first unmoved mover. In both cases the two *viae, quia* and *propter quid*, must be joined together to produce the type of perfect knowing that is characteristic of a *scientia.*[23]

[22] The Latin reads: "Regressus est processus artificialis, scientificus, monstratio ad invicem effectus cum causa, converso medio cum maiore, non eodem modo demonstrationis, nec ad eandem rem quaesiti," *Quaesita aliquot*, fols. 223v-224r.

[23] *Ibid.*, fol. 223v-224r; see also Papuli, 242-54, 275. For a fuller exposition, see

Zabarella and the Regressus

After Balduino, the next great expositor of scientific methodology is Jacopo Zabarella (1533-1589), who brought teaching on the *regressus* to its highest point of development. Before coming to him, however, it is desirable to treat briefly of three other thinkers who ease the transition between the two. The first of these is Girolamo Capivacci, who published his *De differentiis doctrinarum* at Padua in 1562. In it he made the point that Galen's ideas of ways (*viae*) that are resolutive, compositive, and definitive are concerned only with the systematization of knowledge already acquired, whereas the principal means for the acquisition of new scientific knowledge should be divided into the demonstrative, definitive, divisive, and resolutive. The definitive, according to Aristotelian teaching, searches for the genus and specific difference so as to approximate as far as possible the essence of the individual being studied. The divisive then reconstructs the properties of the various species to locate where those of the particular individual belong. The demonstrative is then divided into the three types of demonstration, *quia, propter quid*, and *simpliciter*, but Capivacci denies that any particular one of these can be identified with the resolutive method. Rather the latter includes the entire functioning of the *regressus* and is constituted of two moments: the first is an "ascending" moment from the phenomena to the essential definition, the second a "descending" moment toward the specific phenomena that are being demonstrated in the individual.[24]

The other two thinkers are Bernardino Tomitano (1517?-1576) and Ludovico Boccaferri (1482-1545), both of whom took up and advanced Balduino's teaching that the *regressus* is a concatenation of demonstrations *quia* and *propter quid*, with the first having an analytic function and the second a synthetic. Tomitano, who held the chair of

Giovanni Papuli, *Girolamo Balduino: Ricerche sulla logica della Scuola di Padova nel Rinascimento*, Mandura 1967, 243-74. For Balduino, the two *viae* are no longer two distinct stages but go back and forth in concatenation. The manifestation of the connection between cause and effect, and the articulation of the discourse that is proper to the human way of knowing, occur in the same mental process. This becomes clear in scientific investigation, where the finding of the proximate cause is the work of trial and error. Yet the *quia* and the *propter quid* are two distinctive elements, connected by the reciprocity of the middle term which during the process becomes more and more adjusted and more strictly defined. In the end the effect is no longer a simple fact of nature, but a necessary consequence of the cause: the *ordo mentalis* and the *ordo naturalis* have thus intertwined.

[24] Papuli, 254-5, 275; see also his *Girolamo Balduino*, 44-8.

logic at Padua for years and was Zabarella's teacher, was much preoccupied with the problem of method. For him natural science must use the regressive method of discovery and demonstration based on signs, that is, from particular effects. He is the first to identify the first stage of the *regressus*, demonstration *quia*, as an inductive process, the way of inquiry (*inquisitio*), to be opposed to the second stage, which is deductive. Without a method of induction, for him, there would be no possibility of having a natural science.[25] Boccaferro, who taught at the University of Bologna, was at pains to differentiate the demonstrative induction employed in the *regressus* from that used in dialectics or rhetoric, both of which work on the plane of the contingent and do not arrive at necessary and universal knowledge. He also worked on the problem of circularity to explore various ways of justifying the conversion from effect to cause and from cause to effect in the two types of demonstration.[26]

It is within this general setting that Zabarella set about formulating what was to become the standard version of a logic of discovery and proof identified with the Paduan Aristotelians. A professor at Padua from 1564 to 1589, he wrote numerous works on logic, including an extensive commentary on the *Posterior Analytics* and various treatises on natural philosophy. His definition of the *regressus* is classical and may be stated as follows: "It is a kind of reciprocal demonstration in which, after we have demonstrated the unknown cause through the known effect, we convert the major proposition and demonstrate the same effect through the same cause, so that we know why the effect exists."[27]

For Zabarella logic is practically identified with method, and science itself is nothing more than logical method put to use. For him, the definition of method does not differ from that of the syllogism. Moreover, all scientific progress from the known to the unknown is

[25] In his *Animadversiones aliquot in primum librum Posteriorum Resolutoriorum*, Venice 1574; see *School of Padua*, 48-9; Papuli, 254-5, 276.

[26] The main texts are found in his *De physico auditu liber primus explicatus*, Basel 1571, fols. 13v-14v, and *In duos libros Aristotelis de generatione et corruptione commentaria*, Basel 1571, fols. 93r-113r; see Papuli, 255-6, 276.

[27] The Latin reads as follows: "... est enim reciprocata quaedam demonstratio, qua postquam causam ignotam ex effectu noto demonstravimus, maiorem propositionem convertimus, et eundem effectum per eandem causam demonstramus, ut sciamus quid sit ...," *De regressu*, cap. 1, *Opera logica*, 3d ed., Cologne: Sumptibus Zetzneri, 1597, col. 481C. This edition, which has the same pagination as the Frankfurt 1608 edition, is cited throughout.

either from cause to effect or from effect to cause; the former he calls the demonstrative method, the latter the resolutive. If we progress from one thing to another, neither of which is the cause of the other, there cannot be an essential and necessary connection (*connexus essentialis ac necessarius*) between them and thus no certain knowledge results. Although the demonstrative method, that of composition, is most appropriate in mathematics, where causes are more known than their effects, the resolutive method is characteristic of the natural sciences, where one must start from effects because causes are generally unknown. And since we cannot set out from the unknown, in physics we must employ a kind of secondary procedure, the resolutive method that leads to the discovery of principles. Hence for Zabarella the resolutive method is subordinate and the servant of the demonstrative. The end of demonstrative method is perfect science, knowledge of things through their causes; the end of the resolutive method is discovery (*inventio*) rather than science, since by resolution we seek causes from their effects that we may afterwards know the effects through their causes, not that we may rest in a knowledge of the causes themselves.[28]

Having thus set the stage for his discussion of resolutive method, Zabarella, pursuing a line of thought that was only implicit in Nifo, points out that there are actually two methods of resolution. The one is demonstration from effects, which is efficacious for the discovery of things that are obscure and hidden. The other is induction, which is a much weaker form of resolution and is used for the discovery of something that is not completely unknown yet needs to be made clearer. Induction, for Zabarella, is most helpful for the discovery of principles that are known *naturaliter* and do not require proof through something else. Induction does not prove a thing through something else; rather it reveals that thing through itself. For the universal is not distinguished from the particular in the thing, but only by reason. And since the thing is better known as a particular than as a universal, induction is thus a process from and to the thing itself. That is, it proceeds from knowing the thing in the way it is more obvious to us to knowing it in the way it is more obscure and hidden. On this account, not only are the principles of things known by induction, but also the

[28] Zabarella, *De methodis*, Lib. 1, cap. 2; Lib. 3, capp. 3, 17-18, in: *Opera logica*, cols. 134-138, 226-229, 264-268; texts cited in *Causality*, 144-5, *School of Padua*, 50-2; Papuli, 256-66.

principles of science and of knowing itself, which are said to be indemonstrable.[29]

Zabarella's analysis here outlines an analytic method of discovery whereby ordinary experience is brought to the level of the scientific. He explains this process more fully in his *De regressu*, where he first makes the distinction between two types of knowing, one confused (*cognitio confusa*), the other distinct (*cognitio distincta*), and which he says applies to both knowledge of the effect and knowledge of the cause. A confused knowledge of something is an awareness of its existence (*esse*) without knowing what it is (*quid sit*), whereas a distinct knowledge grasps not only the existence but also the nature of the thing. With regard to particulars, Zabarella further makes the point that it is not necessary that every fact or particular be recorded, since a general principle can be gotten inductively by a careful examination of selected instances or illustrations. This procedure, what he calls demonstrative induction (*inductio demonstrativa*) is only effective in a necessary subject matter wherein things have essential connections with each other. After a certain number of these have been examined, the mind straightaway notices the essential connection, and then, disregarding the remaining particulars, it proceeds at once to bring all the particulars together in the universal.[30]

In connection with this inductive process one might wonder whether Zabarella knew of Nifo's discussion of the work or *negotiatio* of the intellect it requires. In fact he did. After the effect-to-cause stage of the *regressus* has been completed, he writes, before returning from the cause to the effect there must intervene a third intermediate work (*labor*) by which the mind passes from knowing the cause confusedly (*confuse*) to grasping it distinctly (*distincte*). Some thinkers—Nifo is here implied—call this stage a *negotiatio* of the intellect. Zabarella himself thinks of it as a mental examination (*examen mentale*) or a mental consideration (*mentalis consideratio*) of the cause itself: after hitting upon the cause, this consideration leads one to understand what it is (*quid sit*).[31]

From this Zabarella goes on to explain the nature of this *examen mentale*. He indicates that there are two things that help one to know a cause distinctly. One is the knowledge that it is (*quod est*), which

[29] *De methodis*, Lib. 3, cap. 19, *Opera logica*, cols. 268-271; *School of Padua*, 53-4; *Causality*, 145-6.
[30] *De regressu*, cap. 4, *Opera logica*, cols. 484-485; *Causality*, 146-7; *School of Padua*, 56.
[31] *De regressu*, cap. 5, *Opera logica*, cols. 486-487; *Causality*, 147; *School of Padua*, 57-8.

obviously prepares for the discovery of what it is (*quid sit*). The other help, which is a necessary adjunct to the first, is the comparison of the cause discovered with the effect through which it was discovered. At the outset this is not with full knowledge that one is the cause and the other the effect, but simply a comparison of the one with the other. From this examination it comes about that the investigator gradually gains a knowledge of the conditions (*conditiones*) of each; moreover, when the first of the conditions has been discovered this helps to the discovery of another, until finally the cause comes to be recognized as providing the unique explanation of the particular effect.[32]

Despite this detailed analysis of the *regressus*, Zabarella provides no striking examples of how it has been, or can be, used in scientific discovery. He does give two illustrations of the ways Aristotle used the method in his *Physics*, essentially the same as proposed by Balduino, though worked out in more detail. The first is how Aristotle used substantial change to come to the existence of a protomatter (*materia prima*), and then regresses back from this to explain how mutations or transformations occur in the order of nature. The other is how he analyzed series of movers and moveds to come to the existence of an eternal first mover (*primus motor aeternus*), and then regresses back from this to explain all motion in the universe.[33] Unfortunately Zabarella does not examine examples drawn from the mixed sciences, though he offers an excellent analysis of the lunar eclipse in his *De medio demonstrationis*, Lib. 1, capp. 10-13. Also, in his commentary on the first book of the *Posterior Analytics*, cap. 12, he explicitly identifies the way Aristotle uses the *regressus* when explaining phases of the moon and the twinkling of planets. We shall return to this example when explaining Galileo's use of the *regressus* in his astronomical discoveries.

Galileo and His New Science

One might think that Galileo became acquainted with Zabarella's teaching on the *regressus* when he himself began teaching at the University of Padua in 1592. This has been the general supposition of those who have previously connected Galileo's mentions of resolution and composition with the Paduan Aristotelians. As a matter of fact Galileo

[32] *De regressu*, cap. 5, *Opera logica*, col. 487; *Causality*, 147-8; *School of Padua*, 58.
[33] The protomatter example is explained in *De regressu*, capp. 4-5, the prime mover example in *De regressu*, cap. 6: *Opera logica*, cols. 484-492.

already knew of the *regressus* at Pisa and actually used it there in his early studies of motion. How he learned of the method has been fully discussed in the recent literature and need not be repeated here in detail.[34] Galileo probably came upon it quite accidentally in 1587 or 1588 through a contact with Christopher Clavius, the foremost mathematician at the Jesuit university in Rome, the Collegio Romano. Clavius had read one of Galileo's first compositions, *Theorems on the Center of Gravity of Solids*, and called Galileo's attention to what he perceived as a *petitio principii* in its main proof. To brush up on his logic, through Clavius's good graces Galileo obtained a copy of the lectures then being given at the Collegio on Aristotle's *Organon* by Paolo Della Valle. From these he appropriated, in his own hand, two treatises on the *Posterior Analytics*, one on the foreknowledge required for demonstration and the other on demonstration itself. The last question of the latter treatise contains Galileo's exposition of the demonstrative regress.

Della Valle's explanation of the regress follows closely that of Zabarella in his *De regressu*, though there are a few changes of expression in it that are reflected in Galileo's version. The main difference is that where Zabarella distinguishes between knowing a thing first confusedly (*confuse*) and then distinctly (*distincte*), Galileo speaks of knowing it first materially (*materialiter*) and then formally (*formaliter*). The alternate terminologies can be explained simply enough in terms of an intermediate appropriation of Zabarella's teaching by another Jesuit, Giovanni Lorini, who taught the logic course at the Collegio before Della Valle. By tracing successive changes from Lorini to Della

[34] The beginnings of this research are reported in my *Prelude to Galileo: Essays on Medieval and Sixteenth-Century Sources of Galileo's Thought*, Dordrecht-Boston 1981. The basic lines of a solution are sketched in my *Galileo and His Sources: The Heritage of the Collegio Romano in Galileo's Science*, Princeton 1984. Additional details are provided in my *Galileo's Sources: Manuscripts or Printed Works?*, in: G.B. Tyson and S. Wagonheim * (eds), *Print and Culture in the Renaissance: Essays on the Advent of Printing in Europe*, Newark, Del. 1986, 45-54, and my *Galileo, the Jesuits, and the Medieval Aristotle*, in: Collected Studies Series CS 346, Hampshire (UK) 1991. The Latin text of Galileo's logical treatise was published in 1988 (note 2 above); my English translation then appeared as *Galileo's Logical Treatises: A Translation, with Notes and Commentary, of His Appropriated Latin Questions on Aristotle's* Posterior Analytics, Dordrecht-Boston-London 1992. This was accompanied by my *Galileo's Logic of Discovery and Proof: The Background, Content, and Use of His Appropriated Treatises on Aristotle's* Posterior Analytics, Dordrecht-Boston-London 1992, which explains how Galileo made use of the demonstrative *regressus* in his scientific work. Additional reflections will be found in my *Dialectics, Experiments, and Mathematics in Galileo*, forthcoming in a volume on * *Scientific Controversies*, eds. Marcello Pera and Peter Machamer.

Valle to Galileo one can ascertain the intended equivalence of the two sets of terms.[35]

As Galileo presents the teaching it involves two demonstrations, one *quia* and the other *propter quid.* He refers to these demonstrations as progressions (*progressiones*) and notes that they are separated by an intermediate stage. The first progression argues from effect to cause and the second reverses the direction, regressing from cause to effect. For the process to work, the demonstration *quia* must come first, and the effect must be better known than the cause, which initially is grasped only "materially," though later the effect and the cause are seen to be convertible. Then, "having made the first progression," Galileo continues, "we do not begin the second progression immediately, but wait until we come to know formally (*formaliter*) the cause we first knew only materially (*materialiter*)."[36] Following this waiting period or what we have called the intermediate stage, the second progression starts: in it the cause, having been grasped "formally" or precisely as it *is* the cause, and indeed the unique cause in view of the convertibility condition, is shown to be necessarily connected with the effect. Only at this stage is knowledge that is strictly scientific attained, for then one knows the *propter quid*, the proper cause of the effect that is being investigated.

This is the *logica docens* found in Galileo's appropriated treatise on demonstration. The waiting period he inserts as an intermediate stage between the two progressions seemingly corresponds to Nifo's *negotiatio intellectus* and to Zabarella's *examen mentale.* But in the *logica utens* Galileo developed when he put the *regressus* to work in his scientific investigations, this intermediate stage expands far beyond anything previously explained by Nifo or Zabarella.[37] It involves not only time but also work, for testing where experimentation is needed and for computation when mathematics is involved. Predictably, most of Galileo's uses of the *regressus* were in the "mixed sciences," either in mechanics or in astronomy. In the first experimentation was dominant, in the second, computation. On both counts, then, those of experiment and mathematics, the Paduan regress turned out to be

[35] A detailed analysis is given in my *Randall* Redivivus (note 1), 142-5.
[36] *Galileo's Logical Treatises*, 183; Latin edition, 112.
[37] For Galileo's understanding of the difference between *logica docens* and *logica utens*, see my *Galileo's Logic of Discovery and Proof*, 21-6; his teaching on this is essentially the same as Zabarella's, as noted there on p. 24. Balduinus's views on the subject are similar; see Papuli, *Girolamo Balduino*, 89-101.

open to innovation on precisely the points Galileo could exploit. In many cases he signalled its use by a reference, sometimes implicit, to resolution and composition.[38] Only once does Galileo mention the "demonstrative progression," and this in his 1612 analysis of floating bodies.[39] But he gives a very good description of it in his first attempt to formulate a proof for the earth's motion from the tides in 1616. In concluding his proposed proof Galileo explains to Cardinal Orsini how well he has been able to harmonize the earth's motion and the tides, "taking the former as the cause of the latter, and the latter as a sign of and an argument for the former,"[40] which is precisely what one does in the *regressus*.

Full details of how Galileo actually used the demonstrative regress, sometimes unsuccessfully, are given in *Galileo's Logic of Discovery and Proof.*[41] By way of illustration, his most dramatic proofs are those that followed the simple pattern proposed by Aristotle in *Posterior Analytics* I,13 and mentioned at the outset of this study. These are his arguments for the existence of mountains on the moon, for the presence of four satellites orbiting Jupiter, and for Venus's phases as proof of its motion around the sun. All start from some new phenomenon detected through the use of a small telescope. From this all regress to a hitherto unknown cause. In the intermediate stage, all use principles of projective geometry to show the convertibility of this cause with the phenomenon recognized as an effect. Finally, all conclude apodictically for anyone who can verify the data and comprehend the mathematics involved.[42]

[38] Papuli refers to most of these in his *La teoria del* regressus, 268-72.

[39] Galileo Galilei, *Le opere di Galileo Galilei*, ed. Antonio Favaro, 20 vols. in 21, Florence: G. Barbèra Editore, 1890-1909, rpt. 1968, Vol. 4, p. 67. Henceforth reference as GG, as in GG4:67.

[40] GG5:293. The regress involved is analyzed in detail in *Galileo's Logic of Discovery and Proof*, 212-6.

[41] The main astronomical arguments are those for the moon's phases and aspects, p. 195; the mountains on the moon, p. 199; the satellites of Jupiter, p. 202; the phases of Venus, p. 205; the tidal argument of 1616, p. 214; and the tidal argument of 1632, p. 229. The main mechanical arguments are those for determining speeds of fall in various media, p. 242; the dependence of speed of fall on specific weight, p. 248; the variation of speeds in different media, p. 250; the ratios of motions on inclined planes, p. 253; the increase of speed in falling motion, p. 256; the true cause of flotation, p. 271; uniformly accelerated motion in free fall, p. 271; the true cause of cohesion, p. 281; the breaking strength of a beam, p. 283; and the semi-parabolic motion of projectiles, p. 292.

[42] With regard to the use of mathematical proofs in the *regressus*, in an essay published in 1983 Enrico Berti seemed to regard their use as leading only to a dialectical conclu-

The illustrations of the demonstrative regress provided in Galileo's writings are undoubtedly superior to those adduced by Balduinus and Zabarella. It is not easy in the present day to give assent to *materia prima* or to the *primus motor aeternus*. By the same token it is very difficult now to deny that there *are* mountains on the moon, that satellites *do* orbit Jupiter, and that Venus *is* in orbit around the sun. Our knowledge of these hitherto unknown facts shows in a striking way that the *regressus* works, and Galileo deserves great credit for teaching us how it can.

sion. See his *Differenza tra il metodo risolutivo degli aristotelici a la 'resolutio' dei matematici*, in: *Aristotelismo veneto e scienza moderna*, Vol. 1, 453-7. More recently, in his *La teoria aristotelica della dimostrazione nella 'tractatio' omonima di Galileo*, in: M. Ciliberto and C. Vasoli (eds), *Filosofia e cultura, per Eugenio Garin*, Rome 1991, 327-50, Berti has revised this opinion. Having reviewed my recent work on Galileo's *Tractatio de demonstratione* he concedes that these proofs are indeed apodictic. Moreover, with regard to Galileo's statement to Fortunio Liceti in 1640 that, in matters of logic, he has been a peripatetic all his life, Berti remarks that, like many others, he once thought that this was dissimulation, but now he is inclined to believe that Galileo was telling the truth.

XIII

GALILEO'S REGRESSIVE METHODOLOGY, ITS PRELUDE AND ITS SEQUEL

In this essay I discuss Galileo's understanding of regressive methodology as it is set forth in two of my books[1] that provided *entrée* to the theme on which this volume of essays is based, namely, "Method in Sixteenth-Century Aristotle Commentaries." The exposition is basically autobiographical, for I begin with a brief account of my research on two Dominican friars, Dietrich von Freiberg and Domingo de Soto, whose writings led me to studies of Galileo's methodology. This is the "prelude" of my title. I also conclude with a "sequel" in which I offer a few reflections on how Galileo's methods bear comparison with those of two English scientists in whom I have been interested, William Harvey and Sir Isaac Newton. In this way I hope to cast light on Aristotelian methodology not only as it was developed in the sixteenth century but also as it was understood in centuries previous as well as the famous century that followed.

Prelude

Jacopo Zabarella pointed to chapter thirteen of the first book of the *Posterior Analytics* as the place where Aristotle first introduced the demonstrative *regressus*, though not explicitly by that name.[2] Oddly enough Dietrich von Freiberg cited the same chapter of the *Analytics* in

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1 *Galileo's Logic of Discovery and Proof* and *Galileo's Logical Treatises*, cited below in notes 5 and 16 respectively, which were provided to participants of our 1992 Wolfenbüttel seminar by the Foundation for Intellectual History.

2 This was in his commentary on Book 1 of the *Posterior Analytics* in the passage where Aristotle is discussing the non-twinkling of planets and the phases of the moon, which Zabarella identifies as chapter 12 rather than 13. Zabarella has the marginal note *Regressus* at this passage; see his *Opera logica*, Cologne 1597, cols. 836-40, at col. 839A; see also col. 836E-F.

introducing his treatise on the rainbow, *De iride*, written between 1304 and 1311, wherein he proposed to follow Aristotle's canons.[3] The *De iride* is very important for the history of science, for in it, *pace* Descartes and Newton, Dietrich was the first to work out a correct explanation of the primary rainbow, demonstrating its various properties from its causes as he had found these out by experimental investigation. The formal cause is its appearance in the heavens, fixed by measurable parameters, the efficient cause is light radiated from the sun or other heavenly body, and the material cause is the collection of spherical raindrops in which the light rays are reflected and refracted according to optical laws to produce the rainbow's appearance in the eye of the observer. In my 1959 analysis of this work I showed how Dietrich used the twofold Aristotelian method, the *via inventionis* and the *via judicii*, first to discover the causes of the rainbow and then to use them to demonstrate its properties.[4] Thus his was a "logic of discovery and proof," essentially the same as the logic I have now attributed to Galileo.[5] Unfortunately Dietrich's work lay in manuscript for centuries, was not mentioned in print until 1514, and was not transcribed and published until four hundred years later, in 1914. A critical edition finally appeared in Dietrich's *Opera omnia*, edited between 1983 and 1985.[6]

Following that study in late medieval optics and a five-year term as one of the editors of the *New Catholic Encyclopedia*,[7] I began to work on Renaissance mechanics in 1965. I did so at the suggestion of Professor I. Bernard Cohen of Harvard University, who was aware of the fact that a Spanish Dominican, Domingo de Soto, had enunciated the correct law of falling bodies around 1550, some eighty years before Galileo, and thought that someone with my background should study that further. So I worked

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3 See his *De iride et radialibus impressionibus*, ed. Joseph Würschmidt (Beiträge für Geschichte der Philosophie des Mittelalters 12/5-6), Münster 1914, 36.

4 *The Scientific Methodology of Theodoric of Freiberg. A Case Study of the Relationship Between Science and Philosophy* (Studia Friburgensia, New Series 12), Fribourg 1959, 174-248.

5 In my *Galileo's Logic of Discovery and Proof. The Background, Content, and Use of His Appropriated Treatises on Aristotle's "Posterior Analytics"* (Boston Studies in the Philosophy of Science 137), Dordrecht/Boston 1992.

6 In: *Corpus Philosophorum Teutonicorum Medii Aevi*, series II, 4 vols., ed. K. Flasch, vol. 4, (M. R. Pagnoni-Sturlese et al., eds), Hamburg 1985, 95-268.

7 W. J. McDonald et al. (eds.), 15 vols., New York 1967, with supplementary volumes in 1974, 1979, and 1989.

with Cohen at Harvard until 1967, by which time I had assimilated what was then known about the *Calculatores* at Oxford, the *Doctores Parisienses*, and John Herman Randall's "School of Padua," and had started to flesh out the calculatory tradition on the Iberian peninsula, particularly at Salamanca.[8]

In Soto's terminology a motion that is uniformly accelerated in free fall is described as *uniformiter difformis*, so I canvassed every occurrence of that expression I could find in sixteenth-century manuscripts and books, first following a trail left by Pierre Duhem and then striking out on my own.[9] Alexandre Koyré had posed what he called "the enigma of Domingo de Soto," namely, how Soto came to associate *uniformiter difformis* with the motion of falling bodies.[10] Having worked that out fairly well, I started on "a second enigma," how Soto's finding might have been transmitted to the young Galileo.[11] It just happened that Galileo mentions Soto in his early Latin manuscripts, written around 1590, one of which contains the *De motu antiquiora*, his early treatise on motion, and that seemed worth looking into.[12]

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[8] The results of that work are summarized in my *Causality and Scientific Explanation*, Vol. 1: *Medieval and Early Classical Science*, Ann Arbor 1972.

[9] See my "The Enigma of Domingo de Soto: *Uniformiter difformis* and Falling Bodies in Late Medieval Physics," in: *Isis* 59 (1968), 384-401, enlarged and reprinted in my *Prelude to Galileo: Essays on Medieval and Sixteenth-Century Sources of Galileo's Thought* (Boston Studies in the Philosophy of Science 62), Dordrecht/Boston 1981, 91-109, cited more fully in note 13 below. Duhem's ground-breaking work here is his *Études sur Léonard de Vinci*, 3 vols., Paris 1906, 1909, 1913. Fuller details of Soto's teaching are given in my "Domingo Soto's 'Laws' of Motion: Text and Context," in: E. Sylla and M. McVaugh (eds.), *Texts and Contexts in Ancient and Medieval Science: Studies on the Occasion of John E. Murdoch's Seventieth Birthday* (Brill's Studies in Intellectual History 78), Leiden 1997, 271-304. *

[10] In the second volume of R. Taton's four-volume history of science, *The Beginnings of Modern Science*, tr. A. J. Pomerans, New York 1964, in Koyré's entry on "The Exact Sciences," 94-95.

[11] Preliminary results are reported in my "The Early Jesuits and the Heritage of Domingo de Soto," in: *History and Technology*, 4 (1987), 301-320, reprinted in my *Galileo, the Jesuits and the Medieval Aristotle* (Collected Studies Series 346), Aldershot, 1991. See also my "Domingo de Soto and the Iberian Roots of Galileo's Science," in: K. White (ed.), *Hispanic Philosophy in the Age of Discovery*, Washington, D.C., 1997, 113-29 *

[12] The citations may be found in my *Galileo's Early Notebooks: The Physical Questions. A Translation from the Latin, with Historical and Paleographical Commentary*, Notre Dame 1977, 208, 210.

Unraveling that second enigma has taken quite a long time, and in some ways it is still not solved completely. A key figure has turned out to be Franciscus Toletus, who was Soto's favorite student at Salamanca. Toletus later joined the Jesuits and was sent to Rome to teach philosophy at the newly formed Jesuit university, the Collegio Romano. His teaching there was very influential. Tracing that influence has helped greatly toward understanding not only Galileo's mechanics but also the methodology with which he made the discoveries he did. The path I travelled has been documented in a series of books, among which the more important are *Prelude to Galileo*[13] and *Galileo and His Sources*.[14] But the most fruitful line of investigation came when I fixed unambiguously the source of Galileo's logical questions, and was able to publish, with William F. Edwards, the transcription of the Latin manuscript containing them.[15] That is the manuscript now available in English translation, entitled *Galileo's Logical Treatises*.[16] The introduction to that volume sketches all that was known about Galileo's Pisan manuscripts at the time of its writing. The companion volume, titled *Galileo's Logic of Discovery and Proof*,[17] explains the entire logic course from which Galileo's notes were appropriated, and how I see that logic as having informed Galileo's science throughout his lifetime.

Looking over the impressive bibliography Professor Kessler assembled for our seminar awakened in me a feeling of nostalgia, for in my searches through European libraries I have come across many of the books on his list. Countless times I have checked out volumes and then looked through them for expressions that might provide the key to my enterprise. Some of these have shown up in the titles of my writings: *uniformiter difformis*, *Doctores Parisienses*, *regressus demonstrativus, demonstratio ex*

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13 *Prelude to Galileo* (as note 9).

14 *Galileo and His Sources: The Heritage of the Collegio Romano in Galileo's Science*, Princeton 1984.

15 Galileo Galilei, *Tractatio de praecognitionibus et praecognitis* and *Tractatio de demonstratione*, transcribed from the Latin autograph by W. F. Edwards, with an introduction, notes, and commentary by W. A. Wallace (Università di Padova, Centro per la Storia della Tradizione Aristotelica nel Veneto, Saggi e Testi 22), Padua 1988.

16 *Galileo's Logical Treatises. A Translation, with Notes and Commentary, of His Appropriated Latin Questions on Aristotle's "Posterior Analytics"* (Boston Studies in the Philosophy of Science 138), Dordrecht/Boston 1992.

17 As note 5.

suppositione; others, *praecognitio, instrumenta sciendi, resolutio et compositio, impedimenta*, were signposts for which I looked. But I must say it was always like looking for a needle in a haystack, and many times I despaired of ever finding anything. Galileo's Pisan writings were in manuscript, and the more I worked the more I became convinced that he himself had worked from manuscript sources, so that it was not so much in books but in other manuscripts that I would find the connections for which I looked.[18] I shall return to that possibility and its effects on our project toward the end of the essay.

Regressive Method in the *Physics*

In his introduction and in his essay in this volume Professor Kessler has * put his finger on the key problem the foregoing work poses for anyone studying method in sixteenth-century Aristotle commentaries. I have recognized the problem myself in the context of Galileo studies, first when asking how the teachings in MS Gal. 27 relate to those in MSS Gal. 46 and 71 and then when searching for Galileo's *logica utens* throughout the broader range of his writings. The basic problem is the tie, if any, that exists between Aristotle's logic and his physics, between the method he articulates in the *Posterior Analytics* and the way he puts that method to use in the *Physics, De anima,* and *De partibus animalium.* The problem can be posed in the context of the Greek Aristotle, where some scholars, such as Jonathan Barnes,[19] would deny that any such tie exists, or in the context of the Renaissance Aristotle, which was the project for our seminar. More proximately the question can be raised whether this tie existed among the Jesuits of the Collegio Romano in the late 1580's, or even in the works of Galileo's principal source, Paulus Vallius himself. Here I shall attempt to answer those relating to the Jesuits in general. With regard to Vallius, although I have studied his *Logica* I have not had access to his *Physica*, so I will have to postpone a reply until that

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18 Details are given in my "Galileo's Sources: Manuscripts or Printed Works," in: * G. B. Tyson and S. Wagonheim (eds.), *Print and Culture in the Renaissance: Essays on the Advent of Printing in Europe*, Newark, Delaware 1986, 45-54.

19 "Aristotle's Theory of Demonstration," in: J. Barnes et al. (eds.), *Articles on Aristotle: 1. Science*, London 1975, 65-87, revised from an earlier version in *Phronesis* 14 (1969), 123-152.

opportunity presents itself.[20] But I do have copies of the physics courses taught by Toletus and Antonius Menu, who taught them at the Collegio prior to Vallius, and by Mutius Vitelleschi and Ludovicus Rugerius, who taught them immediately after him. My study of all four yields an affirmative answer: there was such a tie, though it is not stated so explicitly as to make it apparent on cursory reading.

The burden of my essay is to review the evidence in those lectures that leads me to that conclusion. For the benefit of those who might experience difficulty with the Latin in which they were given, rather than give the original texts (which are available in the sources I cite), I have translated or paraphrased all of their teachings into English.

Toletus

As has already been mentioned, Franciscus Toletus is important as a link between Galileo's logic and the eclectic Thomism of Domingo de Soto as this was taught at the University of Salamanca. Toletus is also important for publishing the course in philosophy he gave in his early years at the Collegio Romano, among which his *Physica* and *Logica* are relevant for us.

In his commentary on the *Physics* Toletus discusses the mode of proceeding in that discipline in Book 1, chapter 4, where he begins with a review of what Averroes says on the subject. He writes:

> Concerning the natural mode of proceeding, Averroes says that this is in the manner of a discipline and in a demonstrative way. By "in the manner of a discipline" he means the facile and familiar way of proceeding, which is by induction, example, division, and hypothetical syllogism; by "demonstration" he means the three kinds of demonstration, for these include demonstration of a sign, demonstration of a cause, and demonstration in an unqualified way (*simpliciter*). A demonstration of a sign takes place when we demonstrate something *a posteriori* through signs and effects. A demonstration of a cause is a demonstration that proceeds through a cause, but its effect is already known before the cause, as when one first knowing of an eclipse inquires into its cause

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20 Although Charles Lohr lists Vallius' *Physica* as having been printed at Lyons in 1624, and although a censorship report on the manuscript dated 18 September 1621 is preserved in the Roman Archives of the Society of Jesus, there is some doubt as to whether this work ever appeared in print. It is noteworthy that a similar censorship report on Vallius' *Logica* approved it for publication on 24 June 1612, but that book was not published at Lyons until 1622. Since Vallius died in 1622, it could be that plans for the publication of the *Physica*, originally scheduled for 1624, were abandoned after his death. See *Galileo and His Sources* (as note 14), 17-18.

and then through this cause proves it as an effect; such a demonstration only manifests the cause, and does not prove that the effect exists. On the other hand, when the effect is not known before the cause but is first known when grasped through the cause itself, the demonstration is said to be in an unqualified way because it proves the effect and manifests the cause at the same time.[21]

The natural philosopher, Toletus goes on, uses all three types, but the first and second types more frequently because physical effects are more known to us than are their causes. He then ties in this remark with his logic course, saying that he will give an extensive explanation of it in his exposition of the first book of the *Posterior Analytics* and then stating explicitly that for this "the *regressus* is required." As can be seen from Toletus' statements that follow, there is no doubt in his mind that logical doctrine is relevant to work in natural philosophy:

> The philosopher uses all three types of demonstration, but more frequently demonstration of a sign and of a cause because physical effects are more known to us than their causes. We will give an extensive exposition of demonstration of a cause in our exposition of the first book of the *Posterior Analytics*. For I also think, since the effect is known previous to the cause, that the demonstration proves that the effect exists, because the effect is known *particulariter* before the cause, but not with such necessity that, when it is proved through the cause, it is known *universaliter* and necessarily. And for this the *regressus* is required, so that we might come to know most perfectly an effect that otherwise is known imperfectly. Still a twofold demonstration is to be distinguished, both the one that proves through a cause an effect previously known in some way and the one that proves the existence of an effect that is previously unknown in any way, granted that every demonstration that is made proves the effect's existence if it proceeds through a cause. Thus much of the mode of proceeding [fol. 6va].

Turning to Toletus' *Logica* we find the promised exposition of the demonstrative *regressus* in chapter 3 of Book 1 of his commentary on the *Posterior Analytics*, which is devoted entirely to the problem it poses.[22] Toletus formulates the problem of how demonstration is employed in the regress as follows:

> To this difficulty some moderns reply by distinguishing two kinds of demonstration: *propter quid*, from Averroes, second *De caelo* comm. 31 and eighth *Physics* comm. 65; and another which is both of cause and existence, that is, through which we show the cause of the effect and at the same time prove the effect to exist, as when from causes more known to us we proceed to

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21 Franciscus Toletus, *Commentaria una cum quaestionibus In octo libros Aristotelis De Physica Ausculatione, nunc quinto in lucem edita*, Venice 1600, fol. 6rb-va.

22 Franciscus Toletus, *Commentaria una cum questionibus In Universam Logicam Aristotelis*, Venice 1584.

> hidden effects, and this is most powerful (*potissima*). There is another that shows only the cause of the effect but not the latter's existence, and this is demonstration of the cause. And so they admit that a *regressus* is made from a cause to an effect previously known as to its existence, but the *regressus* shows the cause according to which the effect was previously unknown.
> And although this opinion is not to be rejected, it seems to me that one can respond differently and more to the mind of Aristotle, especially from the opinion of St. Thomas, second *De caelo* 59, where he also defends Alexander against Simplicius and explains the proper force of the demonstrative *regressus*. I say therefore that every demonstration proves the effect to exist and at the same time manifests the cause, which Themistius concedes without argument, second *Posterior Analytics*, chap. 2. Therefore note, as we said above, that although the existence of the effect is known before the cause, this is imperfectly and particularly. But after the demonstration we know the effect universally and more certainly and more clearly; Aristotle teaches this expressly in the first *Posterior Analytics*, chap. 24, where he says that even though we might see that a triangle has three angles we still seek a demonstration, because we would not know this universally except through a demonstration. On this account a demonstration always proves the effect to exist, although with a distinction, for sometimes some foreign and imperfect knowledge of the effect comes before. You say: if such knowledge of the effect is imperfect and particular, since it is through this that we arrive at the cause, the knowledge of the cause will also be imperfect. I say: not so, for such knowledge of the cause was the way to discovering the cause; moreover, once the cause has been found, and through other speculations of the intellect, we know the cause more perfectly. Thus it comes about that through the cause we make the regress to knowing universally the effect that was unknown before the cause. And this is what I think about the *regressus* [fol. 167va].

Thus, in the case of Toletus, we have a clear recognition of the use of the *regressus* in natural philosophy, and complementary explanations of regressive methodology in his expositions of both the *Physics* and the *Posterior Analytics*.

Menu

For Antonius Menu we do not have the notes from his logic course but we do have those of his entire course on natural philosophy, including that on the *Physics*.[23] Like Toletus, Menu has a brief exposition of the order or

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23 Antonius Menu, *Quaestiones in philosophiam naturalem datae a Reverendo Patre Antonio Maria Menu Societatis Iesu Sacerdote*, anno 1577, 20 Octobris, Romae, [In libros Aristotelis Physicorum] ... In libros Aristotelis De caelo ... [In libros Aristotelis De generatione] ... [Metaphysica] ... [Colophon:] 6 Octobris 1579, Ueberlingen, Leopold-Sophien-Biblithek, Cod. 138. There is no foliation in this manuscript. Our interest here is in Menu's prologue to the study of natural philosophy, with which the manuscript begins. This is divided into five chapters, entitled respectively 1. Whether there is a science of natural things; 2. On the

method to be observed in natural philosophy; this is found in his prologue to the study of natural philosophy. The method for him is twofold: one is the way of discipline, the order of proceeding in the subject matter, the other is the way of doctrine, the order of proof to be followed in doing so. The order of treatment is in general that sketched by Aristotle in the first book of the *Physics*, that is, from the more universal to the less universal, from principles to causes to properties, and, when treating principles, from the more general and most common to those that are particular. The order of proof, for Menu, is basically one that uses the various types of demonstration sketched by Toletus and explained in the *Posterior Analytics*, though Menu does not name that work nor does he mention the *regressus* in describing the process of discovery and proof. He writes:

> Concerning the method that is to be observed in the parts of natural philosophy, that method is twofold. One is the way of doctrine, and this is nothing more than the order of proof that is followed in the sciences. Another is said to be the way of discipline, which is the mode of proceeding in the doctrine itself.
> The mode of proceeding in the *Physics* is in general from the more universal to the less universal, as in the first *Physics*, text 4, where it says that one should begin with more universals because they are more known. For physics treats first of the principles involved in coming to be, then of the causes and properties of natural things. In the treatment of the principles involved in coming to be it starts from more universals, because it does not first treat of the particular principles of human generation, etc., but explains the general and most common principles of the coming to be of all things generable, and because in explaining questions of common principles and properties it states the more common first, for example, whether the principles are finite or infiite, contraries or not, and so on.

In outlining the order of proof Menu notes at the outset that three kinds of things are studied in natural philosophy. Some are most known, like motion, and they require no proof. Others are partly known and partly unknown, like the definition of nature, and these need some declaration or proof. Finally there are things that are unknown, and these are either unknown to us and known to nature or they are unknown to nature and known to us. If they are of the first type they require demonstration *a posteriori* or, in Toletus' terminology, "of the effect," and if they are of the second type they require demonstration *propter quid* or, in Toletus' terms, "of the cause." It rarely happens in physics that things are

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object of natural philosophy; 3. On the principles, causes, and properties of the subject in general; 4. On the parts of natural philosophy, along with the order and method to be followed in these books; and 5. On the utility and necessity of the *Physics* and some of its uses in the whole of philosophy.

unknown to us and unknown to nature, and thus it will be most rare to find in natural philosophy the kind of demonstration Averroes refers to as *potissima*, Toletus' "in an unqualified way." In Menu's own words:

> The way of doctrine or the genus of proving is manifold. For there are three kinds of things. Some are most known, and so they do not need proof—for example, motion. Others are partly known and partly unknown, and these need either some declaration or proof, as the definition of nature. Others are unknown or occult and these are either unknown to us and known to nature—and these are demonstrated by demonstration *quia* and *a posteriori* through composites and accidents, as Aristotle indicates in the first book of the *Physics*, texts 2 and 3, or they are unknown to nature and known to us, and these are demonstrated by demonstration *propter quid*. For it rarely happens in physics that things are both unknown to nature and unknown to us, and therefore most rarely will one find the demonstration Averroes calls *potissima*, namely, that which shows the *propter quid* and the *esse* of the thing itself.

We may conclude from the way in which Menu describes the demonstration involved in physics that, though the terminology may be slightly different, his teaching is basically the same as Toletus'. Demonstration "of the cause" is precisely the type of demonstration that involves one in the *regressus*. Thus for Menu as for Toletus there is a close methodological link between teachings of the *Physics* and those of the *Analytics*.

Vitelleschi

The next Collegio professor we shall consider, Mutius Vitelleschi, left both a physics course and a logic course, and his introduction to the *Physics* is much lengthier than either Toletus' or Menu's.[24] Vitelleschi further divides the first chapter of the first book into the five traditional texts found in Joannes Argyropylus' edition of the *Physica Auscultatio* that were made available to our seminar, and comments on these individually. Since in his commentary he addresses logical questions along with physical questions, and toward the end specifically addresses the problem of the "more known" as this is presented differently in the *Physics* and the *Posterior Analytics*, I shall make his analysis available in considerable detail.

Vitelleschi begins by noting that, since it is the common opinion of commentators that Aristotle's project for this book is to investigate and

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24 Mutius Vitelleschi, *Lectiones Reverendi Patris Mutii Vitelleschi in octo libros Physicorum et quatuor De caelo*, Romae, annis 1589 et 1590 in Collegio Romano Societatis Iesu, Bamberg, Staatsbibliothek, Msc. Class. 70.

explain the principles of natural things, in these first five texts he discourses on principles and then prefaces a prologue to the whole of natural philosophy. That this matter may be more easily understood, he goes on, we should note that for anything to be put in a science two things must be attended to: first, the order to be followed in treating its subject matter, for example, that in physics we should treat elements before compounds, and second, the instruments that lead us to knowledge of these subjects, for example, demonstration *a priori*, which is the principal instrument, and demonstration *a posteriori*, which is less principal and secondary. In this prologue, therefore, Aristotle proposes three things: first, his intent in the whole of natural philosophy, which is to treat the science and knowledge of natural things, and also in this first book of the *Physics*, which is to treat the knowledge of principles; second, the instruments to be used, which are demonstrations *a priori* and *a posteriori*; third, the order to be followed in the explanations that follow. These matters are taken up in the first three texts of Vitelleschi's commentary, as follows:

> First text: *Cum circa omnes doctrinas.*[25] In this first text there is a kind of syllogism, though Aristotle gives only the major premise. The syllogism is this: In every method or science containing principles or causes or elements, we must know these to know everything contained within the science. For we think we know a thing when we know its first causes, etc. Therefore the knowledge of natural things is had from the knowledge of principles. Aristotle deduces from this that natural science should begin with a knowledge of matters that pertain to its principles. From these statements we can gather Aristotle's intention for the whole of the *Physics*, which is to treat a science of natural things, and for the first book, which is to explain their principles. Also apparent is the most potent instrument to be used in treating a science of natural things, for this is demonstration *a priori*, which proceeds from causes and principles.
> Second text: *Haec autem insita natura.* Since Aristotle has said that we must treat of principles, a person might object that principles are unknown and thus we cannot obtain the beginnings of this treatise from them. To this I say that Aristotle has now moved back to a less principal instrument, demonstration *a posteriori*, and this is what he is speaking of in this text and the next, for it must be used in the investigation of principles. Thus Aristotle solves the objection: there is inborn in us by nature a way of proceeding from things more known to us to those more known by nature, if the latter are less known to us. What is known to us is not always the same as what is known *simpliciter*. So to investigate the unknown principles of natural things we use demonstration *a posteriori*, which proceeds from what is more known to us.

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25 The lemmata here are not those given in Vitelleschi's manuscript but those in the text of Ioannes Argyropylus's translation, which was made available to participants in our seminar.

> Third text: *At confusa primo nota nobis.* Aristotle had said that we should proceed from what is more known to us to what is more known to nature. Now he explains what things are more known, and says that "confused things are first known to us," that is, composites, and in these we first know the more universal notes, and then, when we break them down into the parts and principles of which they are composed, the principles themselves come to be known to us. Thus in this text and the preceding we find the following syllogism: For knowing unknown principles we must make use of what is more known to us; but confused and composites are more known to us; therefore we must use them [fols. 32r-33v].

Having used the expression "more known" and indicating that there is a difference between things more known to us and those more known to nature, Vitelleschi now proceeds to a fuller explanation of the distinction. He does this in texts 4 and 5, which focus on Aristotle's use of the term universal and the precise way in which universals are said to be more known. His comments on these texts read as follows:

> Fourth text: *Iccirco ex universalibus.* Here Aristotle proposes the order of doctrine to be followed and the way matters are to be explained following that order. So he says: We should proceed from things more known to us to those more known to nature, which the order of doctrine also demands, but the universal, since it is a kind of whole (*totum est quoddam*), is more known to us than are singulars; therefore the order of knowing demands that we proceed from universals to singulars, that is, to less universals, which can be said to be singulars when compared to more universals. For sciences do not descend to singulars, that is, to individuals.
>
> Proof of the minor, namely, that the more universal is more known to us than the less universal: Aristotle gathers this first from text 3. For since universals contain their inferiors confusedly as parts, insofar as we first grasp everything in the thing confusedly, whereas we grasp singulars distinctly, it follows that universals are more known to us. And so we must take our start from them.
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> Aristotle proves the same minor in this text and text 5 with three arguments. First: a whole according to sense or, what is the same, a sensible and quantitative whole, what we call an integral whole, is more known to us than its single parts; but the universal is a type of whole, although not one in the proper sense; therefore, just as when the eye sees a line it grasps all of the parts at once but confusedly and indistinctly, and then the single parts distinctly, so the intellect, having seen a composite individual, first grasps in it the more common notes and then gradually distinguishes those that are less universal.
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> The second argument is given in the words with which he begins text five: *quoddam modo sese habent*, that is, in the same way that names are related to definitions. For a name indicates what appears to be a kind of whole indistinctly and confusedly and signifies an entire thing, such as the word circle; the definition, on the other hand, signifies the same thing clearly and distinctly, signifying its parts; but the name, because it designates a confused whole, is more known to us than the definition; thus the more confused universal is more known than its parts.

> The third argument: children, under nature's leadership, first call all men father with a kind of confused knowledge, and then they gradually distinguish their fathers from other men. They do so because they first note those things that are common to their father and to other men, and then they gradually identify those that are proper to their father; therefore more universals are more known to us [fols. 33v-34r].

Up to this point I have been following Vitelleschi's exegesis of Aristotle's text. Here, however, he breaks off from the exegesis to explain how the text he has been explaining is not opposed to related texts in the *Posterior Analytics*. One might think that the two teachings are radically different, for in the *Physics* Aristotle states that universals are more known to us, whereas in the *Posterior Analytics* he says the opposite, that they are least known to us. Vitelleschi attempts to resolve the difficulty in the following terms:

> From this it is apparent that this text is not in opposition to what Aristotle teaches in the first book of the *Posterior Analytics*, text 12, as he there also notes, but where he says that the most universals are least known to us whereas singulars are more known to us. For it cannot be denied that the singular is always the first known. Since our intellect takes the origin of its knowledge from the senses, and the senses perceive only singulars, the intellect knows them first. Indeed, the intellect conceives singulars first in general fashion, apprehending the notes that are common in it, for example, that it subsists by itself, that it is material, that it moves, etc. Then it gradually grasps its other notes, somewhat like parts, dividing them one from the other clearly and distinctly. From this it follows that we can say that universals are in some way the first thing known by the intellect. Not indeed in the sense that universals can be known without singulars being known, but because, after singulars are so grasped by the senses, the intellect is brought by a kind of general knowledge to the notes that are more universal and common in them.
> Briefly, then, to the text of the first book of the *Posterior Analytics* we say that Aristotle is speaking there of the universal *in causando*, as are principles with respect to the conclusion, which he is there treating; for these, since they contain a cause, are more unknown to us than is the conclusion, which states the effect. Here, however, he is speaking of universals *in praedicando*, and the more universal these are, the more known they are to us. The reason is that the accidents we first note in singulars present the more universal notes to us first, not the least universal, as is apparent from experience and is seen in the example of children. For accidents of a more universal type are discerned more generally than accidents of a less universal type. For this reason the mind is more accustomed to them. On this account such universals can also be said to be closer to the senses, because the accidents that they represent are perceived by the senses more often, and thus more readily and more easily. Add that Aristotle is speaking only of a type of general knowledge, for doubtless it is by this knowledge that universals are more known [fol. 34r-v].

Objections can be raised against this reply, however, and Vitelleschi is aware of them. So he proceeds to elucidate it in two alternative ways,

after which he recapitulates what he regards as the main point Aristotle is making in his prologue to the *Physics*:

> But you might object: Aristotle says that we should treat first of principles; but principles, since they are principles *in causando*, are less known to us; therefore it is not true that Aristotle begins with what is more known to us. I reply, first: Aristotle is reasoning in the following way. Whenever we are to begin with principles, since other things cannot be known without them, we first adumbrate confusedly and somewhat grossly the notes that are more universal in these principles and so are more known to us, as, for example, that they are more than one, that they are contraries, that they are not accidents, and so on. Then we distinguish those that are less common and more proper, an order that Aristotle himself follows in an exemplary way in this treatise.
> Second, we can offer a different response as follows. The ordering of the parts of a science is best taken from their subjects. Thus, when Aristotle says that we are to proceed from more universals, he is not speaking of the principles themselves but of the subjects that are more universal *in praedicando*, in such a way that he wishes first to treat of the natural body in general and then of its individual species, and again of compounds in general and then of their species. This, therefore, is what Aristotle is teaching: in the *Physics* we should explain both the principles of natural bodies and the natural body itself and its species. In treating these matters we should proceed from what is more known to us to what is less known. In treating principles we investigate principles themselves in composites and *a posteriori*, and when we have found them and see the natural body as constituted of them, in treating it we first explain the properties of the natural body in general, and then we pursue its individual kinds. Aristotle treats the first of these up to text 3, the second he prepares for in text 4. Other matters that are customarily explained here we have already treated in our commentary on the *Posterior Analytics* [fols. 34v-35r].

Thus far Vitelleschi. Note how, in the passage just cited, he stresses that we must first investigate principles *a posteriori*, and then, when we have found them, we should use them to explain the properties, that is, demonstrate the properties *a priori*, which is just another way of describing the demonstrative *regressus*. Also his many references to demonstration and its types in this prologue to the *Physics* is a clear indication that he is thinking as a logician, a point reinforced by his reference to his commentary on the *Posterior Analytics* at the very end of his exposition. With regard to this reference, what Vitelleschi apparently has in mind is his discussion of Aristotle's division of the "more known" into more known by nature and more known to us when commenting on texts 11 and 12 of the *Posterior Analytics*.[26] His commentary on this

26 Mutius Vitelleschi, *Explicationes in Aristotelis Logicam lectae anno 1588 in Collegio Romano per Professorem Mutium Vitelleschum Societatis Iesu, hodie*

work is rather truncated, and thus, in the copy of his lecture notes that is extant, there is no explicit treatment of the *regressus*, as there is in the commentaries of Toletus, Vallius, and Rugerius.

Rugerius

Vitelleschi followed Vallius in teaching the course on the *Physics*, and immediately after him, our next Jesuit, Ludovicus Rugerius, taught the same course. All three had taught the logic course in the year preceding their teaching natural philosophy, so obviously these matters were fresh in their minds. Of all the lecture notes that have survived, those of Rugerius are the most detailed and meticulous, so we should not be surprised that he addresses the problem in which we are interested more directly than the others. He does so in the second question of his exposition of the first book of the *Physics*, which he formulates as follows: "What kind of *progressus* is involved in knowledge with respect to us and in knowledge with respect to nature, in all discourse but particularly in the *Physics*?"[27] Since *progressus* is a generic term that sometimes is used for the *regressus*, the question itself is a promising indication that Rugerius, like Vitelleschi, will combine logical and physical concerns in his exposition of Aristotle's prologue to the *Physics*.

Rugerius' custom is to divide his questions into a number of sub-questions or *quaesita*. The *quaesita* for this question he now enumerates as follows: (1) What does it mean to be more known to us and more known to nature; (2) Can the same thing be more known to us and more known to nature; (3) What kind of universal is Aristotle discussing in this place; (4) Is it true that what is more universal is more known to us; (5) Why is it said to be natural for us to proceed from what is more known to us to what is more known to nature; and (6) What is the way of proceeding in these matters in the *Physics*? All of these questions are obviously of great interest, particularly the fifth, where Rugerius explicitly mentions the demonstrative *regressus*. Obviously we cannot treat all of the queries in equal detail. In what follows I shall simply paraphrase the

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Societatis eiusdem Praepositum Generalem, a Torquato Riccio eius discipulo conscriptae, Rome, Biblioteca Apostolica Vaticana, Lat. Borgh. 197.

27 Ludovicus Rugerius, *Commentariorum et quaestionum in octo libros Physicorum tomus, traditus in Collegio Romano anno Domini 1590*, Bamberg, Staatsbibliotek, Msc. Class. 62-3.

more important elements in each of Rugerius' replies, starting with the first two *quaesita*, which deal with the concept of the "more known":

> *More known to us / more known to nature.* One of the more difficult problems in understanding Aristotle's prologue to the *Physics* is what he means by the distinction "more known to us" vs. "more known to nature." Here Averroes makes the point that elements are more known to nature than are composites, and they are less known to us because we do not make composites the way nature makes them. Therefore, just as with artifacts the artificer knows the things from which artifacts are composed before the composites themselves, so also nature seems to known the things from which natural things are composed, namely, elements, before composites, just as we ourselves would if we were to make them. Some commentators find fault with Averroes for this because he seems to endow nature with intelligence, but actually it is an ingenious and appropriate metaphor. What it intends is not that nature *is* intelligent, but that nature would have this type of knowledge if it *were* intelligent. Thus the definitions to be preferred are the following: "more known to nature" means to be that on which other things depend for their being or coming to be, thus implying the notions of priority and dependency, whereas "more known to us" means whatever is sensed or comes closer to our senses.
>
> *Can the same thing be more known in both ways.* The next query is whether the same thing can be more known to us and to nature. From the foregoing definitions this would appear to be possible if something could be found that is closer to sense and at the same time is that on which another depends in being or becoming. According to Averroes there are no such things in the order of nature, or at least very few, and Aristotle seems quite clear on this. The opposite opinion is that of Pererius, who disagrees with Averroes here and gives examples of such entities that he takes from his own theory of knowledge. Pererius is misguided in this. In physics the same thing can never be more known to nature and to us; rather the contrary is true, for what is more known to nature is always less known to us, and vice versa. The proof: knowledge for us always begins with singulars and composites whereas that for nature works in the opposite direction [fols. 32-39].

The Pererius who is mentioned here is one of Rugerius' fellow Jesuits, Benedictus Pererius, who had taught philosophy at the Collegio Romano from 1561 to 1566, before moving to theology in 1567. Pererius' reading of Aristotle here was apparently influenced by Pererius' idiosyncratic view of universals, with which Rugerius disagrees. He explains the matter more fully in his replies to the third and fourth *quaesita*:

> *More universal / more known to us.* More universal, in this context, means the more universal note in any individual sensed by us. The reason why the more universal is better known than the less universal is not because it is diffused and dispersed among more individuals, as Pererius argues, but because it is more material, for all knowing with respect to us presupposes the materiality of things. Hence it is that from any sensible accident we always start with a material note and go on from that to grasp the less material. And the reason

> why material notes are more known to us is because they have more affinity to us, since we ourselves are more material and corporeal.
> *More universal notes in singulars.* Here the text that is of most interest is text 5 of the first book of the *Posterior Analytics*. According to St. Thomas Aquinas, when Aristotle says there that universals are least known to us whereas singulars are more known, he is comparing universals, that is, natures abstracted from particulars, with particulars and singulars, and in this sense what Aristotle says is true. But when we say that universals are more known to us, we mean something else, namely, that there are more universal notes in one and the same material individual and that these more universal notes are what we first grasp in it. Yet Aristotle is right in maintaining that the more universal is the more unknown, because it is more abstracted from the senses and farther away from singularity. So, even though in any singular it is easier to know a more universal note, when we take our minds away from singulars it is more difficult to grasp universals, and particularly genera rather than species, because as so abstracted they are more distant from the individual [fols. 39-44].

This brings us to Rugerius' fifth query, where he investigates what it means to say that it is "more natural" to proceed from what is more known to us to what is more known by nature. Here everything depends on the precise meaning one attaches to the term nature. Rugerius distinguishes five different meanings of this term, the first three of which he characterizes as follows:

> *What is more natural for us.* The fifth query inquires why it is said to be natural to proceed from the more known to us to the more known to nature, and not vice versa. One may answer this by distinguishing five different senses of the word natural and showing how, in four of the five meanings, the procedure advocated by Aristotle cannot be said to be natural. In Aristotle's fifth meaning it can, and this then forms the basis for an affirmative answer.
> The *first* meaning of a natural way of proceeding is similar to the mode nature takes in producing things, as explained in the first query, and since this is the opposite of the way we understand things, in this sense our mode of knowing is not natural. A *second* meaning of natural is what is congruent with or agreeable to the nature of things, and this mode would require, for example, that knowledge of an accident be dependent on knowledge of substance, whereas here too we proceed in the contrary direction, from knowledge of accidents to knowledge of substance.
> In a *third* meaning natural is said to be what every nature seeks, and even in this way this process is not said to be natural, for man desires by nature to know scientifically, and to know scientifically is to know a thing through its cause. Hence, because the more known to nature are the causes of the things that are more known to us, our intellect does not arrive at this knowledge except through the demonstrative *regressus*. Using the regress, through knowledge in relation to us we arrive at knowledge of causes, and then by going back from them we come to knowledge of effects, and it is this latter knowledge that is knowledge according to nature.

Note here, in explaining the third meaning, that Rugerius makes reference to the demonstrative *regressus* and notes how it is a natural way of proceeding for those who desire to know scientifically. Although he does not say so here, he has a thorough treatment of the *regressus* in his questions on the *Posterior Analytics*,[28] so there can be no doubt as to how he understood the term.

The remaining two meanings of the term natural Rugerius then explains as follows, concluding with a brief note on connatural ways of gathering knowledge that normally precede scientific understanding:

> The *fourth* meaning of natural is anything that is so proper to the nature of man that he cannot be diverted or torn from it. Even in this sense the process is not said to be natural, for though unlearned and ignorant men proceed in this way in their knowledge, those who are expert and learned in the scientific and demonstrative mode of knowing proceed from things more known to nature to those more known to us, and thus it is not contrary to man's nature to do so.
>
> That leaves only the *fifth* meaning, and this is the one that permits an affirmative response. The process of which Aristotle speaks is natural in this sense, that natural means what is done only under "nature's leadership"[29] and without the guidance of any discipline, for this is the process we use when led by nature *before* we have been instructed in various disciplines. This happens in the following way. When we are stimulated from the admiration of effects to investigate the natures of things, and are led by nature in our examination of singulars, we are able to form concepts at the lowest level of universality by comparing things and noting similarities; from such concepts we ascend to the more universal, and so on until we arrive at the most universal. Again, from diligent inspection of composites we discern their parts and the parts of the parts until we come to the ultimate elements. Finally, again under nature's leadership, when we perceive a singular thing we conceive its most universal notes, and then we descend to the less universal. This is apparent in the things we know best, for we first know it to be a being, then a body, then an animal, and then a man, and in children who know their own fathers they first know them as men rather than as particular men, and so they call all men father. And in knowing all these things we proceed in a direction opposite from the way we do in science. For in science, omitting such knowledge of singulars, with which science is not concerned, we proceed from the most universal to the least so as to arrive at the lowest species; again, we proceed from the most basic simples to the things that are composed of them.
>
> And yet, many sayings of Aristotle attest that there is a connatural way of gathering knowledge *prior to* attaining science. For example: When a sense is lacking there can be no science of matters that pertain to that sense. Again: All sciences and arts take their origin from the senses and from memories and

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28 Ludovicus Rugerius, *Commentariorum et quaestionum in Aristotelis Logicam tomus traditus in Collegio Romano anno Domini 1589*, Bamberg, Staatsbibliothek, Msc. Class. 62-1 and 62-2.

29 This is Vitelleschi's expression; see above, 241.

> experiences. Yet again: Our intellects never think of anything without adverting to phantasms. And yet again: The concepts on which intellection is based are not phantasms, but without phantasms they would not exist. All of these statements attest to a connatural mode of knowing that provides the basis for science, even though it is not scientific knowing itself.

With these prenotes explained, Rugerius comes to his final query in this question, whether in physics we *do* proceed from the more known to us to the more known to nature.[30] His answer is somewhat surprising, but of key importance for this seminar. In general, he says, we do not: the universal mode of proceeding in physics is the scientific mode, from the more known to nature to what is more known to us. But sometimes, in certain parts of the discipline, occasionally, for greater facility, we begin with what is more known to us to investigate what is more known to nature. The example he gives from the *Physics* is in the eighth book, moving from motion, which is more known to us, to investigate the First Mover, which is more known to nature. (It is noteworthy that Zabarella gives an example from the first book, moving from motion to *materia prima*, which is also more known to nature.) Rugerius cites also the second book of *De anima*, where Aristotle treats sight before touch, though touch is prior by nature to sight and extends to more objects. So the surprising result of Rugerius' analysis (and it is congruent with views expressed in the other notes I have analyzed) is that regressive methodology, which begins with what is more known to us, is not the universal method followed in natural science. It does have very important uses, however, as should be clear from the many illustrations I have given of how Galileo employed it in my *Galileo's Logic of Discovery and Proof.*

Regressive Method in the Study of the Soul

I have just mentioned Rugerius' reference to the second book of *De anima*, but thus far have said nothing of Jesuit teachings on the mode of proceeding in the study of the soul. Apart from the introductory text of the *Physics*, our seminar also focused on the introductory portions of the *De anima* and the *De partibus animalium.* Before concluding, therefore, it might be well that I make a few remarks about regressive methodology and how our Jesuit authors saw Aristotle employing it in the study of living organisms.

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30 Ludovicus Rugerius, *In octo libros Physicorum* (as note 27), fols. 47-8

As noted in my previous writings, Rugerius' complete course on philosophy is preserved in a series of codices now at the Staatsbibliothek Bamberg; one of these codices contains his commentary and questions on *De anima*. Curious about this, I arranged to spend a few days in Bamberg on my way to the seminar, and while there I copied out his introduction to that work. What follows is a paraphrase of that introduction.[31] Although Rugerius does not mention the *regressus* in the portion I copied, his teaching there is also quite congruent with a regressive methodology.

Rugerius begins by noting the procedure one should follow when analyzing the principles and the properties of living bodies:

> To have perfect knowledge of the proper subject of a science one must have knowledge of the subject's nature or internal principles and also knowledge of its properties. The entire treatise on the living body (*corpus animatum*) will thus have two parts, one dealing with its principles, the other with its properties. But the internal principles are two: one is its proper matter, that is, its parts, both homogeneous and heterogeneous; the other is its form, that is, its soul. The matter or parts are explained in *De partibus animalium*, with those parts relating to movement being additionally treated in *De incessu animalium*. The form is treated in the three books of *De anima*. Then the properties and operations of the living body are covered in the remaining books, the *Parva naturalia* [3-4].

With this as a preamble, Rugerius sketches the ordering of the various biological works in the Aristotelian corpus that should be followed for the implementation of these principles:

> When putting order into the books on living things one should locate the books of *De historia animalium* first. These books are *not* a part of natural science, for they explain the accidents and operations that are known from experience and sense alone, without assigning causes for them. Yet Aristotle proceeds in these historical books in the same way as in the others: in the first four books things that pertain to their matter and parts, in the remaining books things that pertain to their operations and accidents. There is no discussion of the soul in them, since the soul cannot be perceived by the senses.
> Immediately following the *De historia animalium* come the books *De partibus animalium*, for with these Aristotle begins his scientific treatment of the living body. He does so by treating the parts of animals demonstratively (*demonstrative*), assigning the final cause of each part, that is, the operation and function for which nature uses it as a proper instrument. But in animals there are three important kinds of parts: some look to the animal's *esse* or *bene esse*, and these are mainly explained in the books *De partibus animalium*; others look to the animal's movement, and these are explained in *De incessu animalium*; and yet

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31 Ludovicus Rugerius, *Commentariorum et quaestionum in tres libros De anima tomus, traditus in Collegio Romano anno Domini 1592*, Bamberg, Staatsbibliothek, Msc. Class. 62-6. Page numbers are given in square brackets.

> others look to the way the animal generates offspring, and these are explained in the books *De generatione animalium*.
> After all these matters have been demonstrated, then one should turn to the study of the animal's form, to the three books of *De anima*. These are concerned with the soul considered in itself, not with its operations except insofar as these may be necessary for understanding the soul's nature. Thus, the books *De anima* focus on the form of the living body considered precisely in its nature and as it is a constitutive principle of the *corpus animatum*. This completes the defining process, as it were, and prepares for the scientific demonstration of the properties and accidents that are proper to living things [4-5].

Having completed this comprehensive outline, Rugerius draws his discussion to a close by remarking on the scientific content of the *De anima* and how this work is intimately related to the other treatises contained in Aristotle's *Parva naturalia*. He writes:

> It turns out that some scientific reasoning is found in the *De anima* itself, but the arguments there are based on imperfect knowledge, that is, on common opinion, and this suggests a fuller treatment elsewhere. The "elsewhere" is in the books of the *Parva naturalia*. The accidents and operations that are considered in these books are of two kinds: some are common to all living things; others are proper to animals. Again, accidents that are proper to animals are of two kinds: some follow on the nature of the genus absolutely taken; others follow on the ultimate nature and differentia of the species. Of the first kind is sense knowledge, of the second, movement [5-6].

Rugerius does not go into detail on how demonstrations are made of either of these operations. Even from this overview, however, it is easy to see how Rugerius recognizes how Aristotle's procedure in the study of the *corpus animatum* parallels the study of the *corpus naturale* in the *Physics*. In each case we start with details that are closer to sense, and then, "under the leadership of nature," come to the universal notes of particular parts, and finally to the most universal notes—in this case those that characterize the soul itself. This is essentially a regressive methodology, although in his exposition Rugerius does not use the term *regressus*. Once we have regressed to first principles in this way, we are prepared to demonstrate the functions and operations that are proper to living things, first in their generic modalities, then in more and more specific detail, until we ultimately descend to the *infima species* of living things in their almost infinite variety.

The answer to Professor Kessler's query with which I began this discussion of regressive method should, then, be clear. Within the context of the Aristotelianism that was taught at the Collegio Romano in the 1580s and 1590s there was a close tie between the logic course and the courses

offered in natural philosophy. This is true in general, and it is also true of the special tract on the demonstrative *regressus*. The teaching notes of Toletus, Menu, Vitelleschi, and Rugerius bear this out with regard to their courses on the *Physics*, and Rugerius' introduction to his course on the *De anima* explains in a general way how regressive methods had to be employed not only in that work but also in the *De partibus animalium*. With regard to the *De anima*, Toletus' introduction to that work, which I have not discussed here, coheres well with Rugerius' analysis. From the viewpoint of what is more known by nature Toletus argues that the *De anima* should precede the *De animalibus*, but from the viewpoint of what is more known to us, the *De animalibus* should precede the *De anima*, which is precisely what a regressive methodology would demand.[32]

Sequel

With this I come finally to my sequel. I entitled my interpretative volume on Galileo's logic *Galileo's Logic of Discovery and Proof*. By that title I intended to show that, though the concluding part of the demonstrative regress employs a logic of proof, the more important part is that with which it begins, a logic of discovery. In the *Logica Utens* part of the volume I provided example after example to show how Galileo employed, at least implicitly, his paradigm of the demonstrative *regressus* to make the claims he did for the "new sciences" he was so intent on discovering. To return to the prelude of this essay, I could fit into that same paradigm the method of resolution and composition that was used by Dietrich von Freiberg in the early 1300's to discover the correct optical explanation of the primary rainbow.[33] I could also incorporate into that paradigm, as I did partially in *Galileo's Logic of Discovery and Proof*, the work of Domingo de Soto on motions that are *uniformiter difformis*, which was to lead eventually to Galileo's formulation of the law for free fall in naturally accelerated motion.[34]

If space permitted I could go further and show how the same pattern of thought can be discerned in the great classic of William Harvey, *On the*

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32 Franciscus Toletus, *Commentaria una cum Quaestionibus in tres libros Aristotelis de Anima*, Cologne 1615, fols. 5ra-6ra.

33 *Scientific Methodology* (as note 4), 132-248.

* 34 "Domingo de Soto and the Iberian Roots of Galileo's Science" (as note 11); see also my "Duhem and Koyré on Domingo de Soto," *Synthese* 83 (1990), 239-60, and *Prelude to Galileo* (as note 9), 192-252 and 303-19.

Motion of the Heart and Blood.[35] Harvey, of course, was a professed Aristotelian who had studied at the University of Padua, where teaching on the *regressus* had reached its fullest state of development. And, even more surprisingly, I could show how Sir Isaac Newton used the same regressive methods, those of resolution and composition, to explain the colors of the prism, the famous *experimentum crucis*, in his letter to the *Philosophical Transactions* of 1672.[36] These are works of scientific discovery where the investigator starts with what is more known to us to come eventually to a knowledge of its cause, to what is more known to nature. I analyzed all of these cases in a work published in 1972, the first volume of my *Causality and Scientific Explanation.*[37] I remember calling this to the attention of Charles Schmitt once, when he was lamenting the lack of appreciation for Aristotle in the present day, and he replied, "Yes, I know that, but no one cites your work."

Perhaps now I have a second chance. As the programme sketched by Professor Kessler and other works in this volume is filled out, fuller accounts of Aristotelian method will surely be found in Renaissance commentaries. But I am not too sanguine about this prospect, for my own studies of Jesuit printed works thus far have turned up very little—it is mainly in manuscript sources that I have had the greatest success. Many of the books in Kessler's bibliography are standard expositions of Aristotelian natural philosophy, which means that they mainly follow the order of doctrine, going from what is more known to nature to what is more known to us. Science textbooks in the present day do the same: they start with the principles of the discipline and proceed from these to explain the generally accepted conclusions. So in textbooks there is little room for novelty, now as in ages past. But the Renaissance may prove to be

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35 See my *The Modeling of Nature: Philosophy of Science and Philosophy of Nature in Synthesis,* Washington, D.C., 1996, 350-5, 396-400; also my "Three Classics of Science. The Reviews of Three Great Books: Galileo, *Two New Sciences*; Gilbert, *The Loadstone and Magnetic Bodies*; and Harvey, *The Motion of the Heart and Blood*," in: *The Great Ideas Today 1974*, Chicago 1974, 211-72.

36 *Scientific Methodology* (as note 4), 270-8. For Newton's knowledge of Renaissance Aristotelianism, see my "Newton's Early Writings: Beginnings of a New Direction," in: G. V. Coyne et al. (eds.), *Newton and the New Direction in Science*, Vatican City 1988, 23-44, and "Aquinas and Newton on the Causality of Nature and of God: The Medieval and Modern Problematic," in: R. J. Long (ed.), *Philosophy and the God of Abraham*, Toronto 1991, 255-79. See also *The Modeling of Nature* (as previous note), 355-363, 400-409

37 Cited in note 8 above; see 94-103, 135-9, 184-93, and 194-210.

different, and, of course, we never know what will turn up when we become seriously involved in textual research.

XIV

Dialectics, Experiments, and Mathematics in Galileo

Perhaps it is not out of place, in a book devoted to scientific controversies, to begin this chapter with a long-standing controversy over Galileo and his methodology. This is not so much a scientific controversy as it is one relating to the history of science, but it is instructive for the light it can shed on how scientific controversies are ultimately resolved. I refer to the *Methodenstreit* initiated by Ernst Cassirer (1922), developed by John Herman Randall, Jr. (1940, 1976), and contested ever since by a host of writers including Neil Gilbert (1963), William Edwards (1983), Adriano Carugo and Alistair Crombie (1983), and myself (Wallace, 1984). The point of the controversy is whether or not Galileo was influenced by the Paduan Aristotelians, in particular, Jacopo Zabarella, when developing the new sciences for which he is justly famous. My contention now is that the lengthy controversy has finally been resolved: Galileo indeed *was* influenced by Zabarella, but in a novel way not foreseen by any of the protagonists before Edwards and I published the Latin text of his *Tractatio de demonstratione* (Galilei 1988; Berti 1991). The *Tractatio* occupies a major part of MS Gal. 27, entitled *Dialletica,* which had been sitting for many years in the Biblioteca Nazionale Centrale in Florence. It was known to Antonio Favaro, the editor of the National Edition of Galileo's works, but he did not think it worth transcribing for his edition, and so it has remained unknown to scholars for over four centuries (GG9: 273–282).[1] The designation written on the manuscript, "*Dialettica,*" explains the "Dialectics" in my title.

The path leading from Zabarella to Galileo surely was not easy to foresee. Zabarella's teaching came, not by way of an anticlerical Averroist interpretation of Aristotle, as Cassirer and Randall had maintained, but via Jesuit professors of the Collegio Romano who read Aristotle with the eyes of Aquinas rather than those of Averroes

(Wallace 1988). And this was especially fortuitous in that it enabled Galileo to import, within the general framework provided by Aristotle's *Posterior Analytics,* both experimental and mathematical techniques that would become the hallmark of his future scientific work.[2]

Galileo's treatise on demonstration was appropriated with only slight modifications from a complete course on logic and methodology offered at the Collegio Romano by the Jesuit Paolo Della Valle in 1588 (Wallace 1992b, pp. 27–40). The last question of the treatise is devoted to the *regressus demonstrativus* or demonstrative regress, the distinctive methodology of the Paduan Aristotelians. Della Valle had in turn appropriated the teaching from the logic text of Zabarella, the main proponent of the method at the University of Padua. Della Valle did so through the intermediary of another Jesuit, Giovanni Lorini, and this serves to explain a few terminological changes along the line, but the doctrine remained the same nonetheless (Wallace 1988, pp. 143–145). Thus, there can be no doubt that Galileo understood Paduan methodology. The bulk of this chapter is devoted to showing how he employed what it taught, its *logica docens,* in his scientific writings, and how this teaching enabled him to make several discoveries that were very much controverted in the early seventeenth century. These related not only to the novelties in the heavens but also to the new science of motion that occupied him to the end of his life.

The Demonstrative Regress

As Galileo presents the teaching, it involves two demonstrations, one "of the fact" and the other "of the reasoned fact" (Galilei 1988, pp. 108–113; Wallace 1992b, pp. 180–184). Galileo refers to these demonstrations as "progressions" and notes that they are separated by an intermediate stage. The first progression argues from effect to cause, and the second argues in the reverse direction, thus "regressing" from cause to effect. For the process to work, the demonstration of the fact must come first, and the effect must initially be more known than the cause, though in the end the two must be seen as convertible. The intermediate stage effects the transition from it to the second demonstration. The transition itself involves time and work, for testing when experimentation is needed and for computation where mathematics is involved, so that the causal connection can be made clear and precise. The result is then seen in the second progression, when the cause, having been grasped "formally" or precisely as it *is* the cause, and indeed the unique cause in view of the convertibility condition, is shown to be necessarily connected with the effect. Only at this stage is knowledge that is strictly scientific attained, for then one knows the reasoned fact, the proper cause of the effect that is being investigated. The entire process, known as the demonstrative *regressus,* may be schematized as follows:

First progression: from effect to cause—the cause is materially suspected but not yet recognized formally as the cause. This generally presupposes that the effect is more known to the senses than the cause and that it awakens interest or curiosity, thus serving as the starting point of the investigation. At the end of this progression the cause comes to be suspected as plausible, for example, known "materially," as really existing, thus as the terminus of a "demonstration of the fact" (Latin *demonstratio*

quia, Greek *to hoti*), but known only in a material way and not yet as necessarily connected with the effect.

Intermediate stage: the work of the intellect, testing to see if this is a cause convertible with the effect, eliminating other possibilities. This usually requires a period of time, during which the work is that of the mind (*negotiatio intellectus*), not the senses, although sensible experience plays an important and essential part. Basic to this stage is a *mentale ipsius causae examen* (literally "a mental examination of the cause itself"), where the Latin *examen* corresponds to the Greek *peira,* a term that is the root for the Latin *periculum* (meaning test) or *experimentum* (meaning experiment or experience). The main task is thus one of testing, for example, investigating and eliminating other possibilities, and so seeing the cause as required wherever the effect is present. At the end of this period, the cause is grasped "formally" by the mind, that is, precisely as it is the cause, and the unique cause, of the particular effect.

Second progression: from the cause, recognized "formally" as the cause, to its proper effects. At this stage the necessary connection between cause and effect is grasped. The cause is seen as ontologically prior to the effect and thus as more knowable in itself, even though the effect is more apparent to the senses. The cause is also seen to explain the effect, for example, to give a proper reason why the phenomenon appears the way it does. On this account the second progression constitutes a "demonstration of the reasoned fact" (Latin *demonstratio propter quid,* Greek *to dioti*).

In Zabarella's account the intermediate stage, the work of the intellect or the *examen mentale* with its testing procedures (note the Greek *peira,* source of the Latin *periculum*), carries a heavy burden (Zabarella 1597, p. 486; Olivieri 1978, pp. 164–166). Charles Schmitt (1969) has made a detailed study of Zabarella's use of the term *periculum* or experiment as compared with Galileo's use of the same in his early writings. Surprisingly, Zabarella turns out to be more the empiricist than Galileo. Furthermore, in his 1597 commentary on the *Posterior Analytics,* Zabarella identifies the precise point at which Aristotle himself employs the regress (cols. 836–840). This is in his study of the heavenly bodies, where Aristotle reasoned to the facts that the moon is a sphere and that the planets are closer to the earth than the fixed stars (*Posterior Analytics* I.13, 78a31–b12). Both of these demonstrations pertain to the "mixed science" of astronomy, which uses mathematics to explain the phenomena of the heavens (Lennox 1986; Wallace 1992a, pp. 107–111). On both counts, then, experiment and mathematics, the Paduan regress was open to innovation on precisely the points that would be exploited by Galileo.

Early Experiments with Motion

To turn now to how Galileo made use of this teaching, his *logica utens,* I examine first his preliminary studies of motion at Pisa in 1590, shortly after he had written the *Treatise on Demonstration,* then turn to his discoveries with the telescope, and after that examine his advanced studies of motion at Padua. My concern is to show how in all these cases Galileo employed the demonstrative regress, though in varying ways as dictated by the subject matter. Galileo does not identify the regress as such in his writings, mentioning the "demonstrative progression" (*progressione dimostrativa*) only

once, in his 1612 analysis of floating bodies (GG4:67.23). This is not unusual, for the Latin *regressus* has no counterpart in Greek, and Aristotle himself did not use the term. Nor did Aristotle identify syllogisms in his scientific treatises, but this is no sign that he failed to employ them.

Perhaps Galileo's greatest innovation in the study of motion was his use of the inclined plane to slow the descent of bodies under the influence of gravity. The basic insight behind this experiment is found in chapter 14 of Galileo's early *De motu,* "On motion" (GG2:296–302; Drabkin and Drake 1960, pp. 63–69). The *De motu* was composed in 1590, the year after the *Treatise on Demonstration,* and is now conserved at Florence in MS Gal. 71. If the weight of a body can be decreased by positioning it on an incline, thought Galileo, its velocity down the incline will be proportionally slowed. The demonstration he offers is geometrical, but it invokes several suppositions and on this account may be seen as a demonstration *ex suppositione.* If these suppositions are granted, the conclusion follows directly: the ratio of speeds down the incline will be as the length of the incline to its vertical height, because the weight of the body varies in precisely that proportion. His reasoning process here, arranged in the form of the demonstrative regress, is as follows (from Wallace 1992a, pp. 251–255):

First progression:

Effect: Heavy bodies descend along planes inclined to the horizontal more swiftly the greater the angle of inclination.

Cause: Their heaviness on the incline increases with the angle of inclination.

Intermediate stage: Geometrical analysis shows that the ratio of the force required to overcome weight on an incline to that required to overcome weight vertically is as the ratio of the vertical height to the oblique distance along the incline (GG1:298).

Suppositions: (1) that heavy bodies move downward by reason of their weight (*gravitas*), and thus their speed of fall is directly proportional to their weights (GG1:262).

Again: (2) that there is no accidental resistance (*nulla existente accidentali resistentia*) occasioned by the roughness of the moving body or of the inclined plane, or by the shape of the body; that the plane is, so to speak, incorporeal, or at least that it is very carefully smoothed and perfectly hard; and that the moving body is perfectly smooth and of a perfectly spherical shape (GG1:298–299).

Further: (3) under such conditions, that any given body can be moved on a plane parallel to the horizon by a force smaller than any given force whatever (GG1:299–300).

Second progression:

Cause: The weight of a heavy body on an incline is to its vertical weight as its vertical height is to the length of the incline.

Effect: The ratio of its speeds down the incline will be as the ratio of the length of the incline to its vertical height (GG1:298).

Galileo uses the term *periculum* for test or experiment five times in this treatise (Schmitt 1969, pp. 114–123). One occurrence is in connection with his first supposition in the schema, the Aristotelian principle that speed of fall (*V*) is directly proportional to the falling body's weight (*W*). Galileo says that if one performs the *periculum* or experiment these ratios will not actually be observed, and he attributes the discrepancy to "accidental causes" (GG1:273; Wallace 1983). Another place is in connection with the third supposition. Here Galileo states that one should not be surprised if a *periculum* or experiment does not verify this, for two reasons: external impediments prevent it, and a plane surface cannot be parallel to the horizon because the earth's surface is spherical (GG1:301). But if these difficulties can be overcome, the proof will be valid on the basis of the second supposition.

The mathematical argument that forms the basis of Galileo's proof is shown in Figure 6.1. Here is the familiar circle used to analyze the lever in Aristotle's *Mechanical Problems,* a work not known until the Renaissance. Orthogonal lines, BF ⊥ HG and BL ⊥ NO, make evident that the inclined plane actually obeys the law of the lever, and thus a body's weight on the incline (*W*), and consequently its speed downward (V), is diminished in the ratios BK/BF and BM/BL, as indicated in the figure.

Another example of the demonstrative regress occurs in chapter 19 of the *De motu* treatise, where Galileo uses it against Aristotle to explain why bodies increase their speed, or accelerate, during fall (GG2:315–323; Drabkin and Drake 1960, pp. 85–94). The argument may be diagrammed as follows:

First progression:

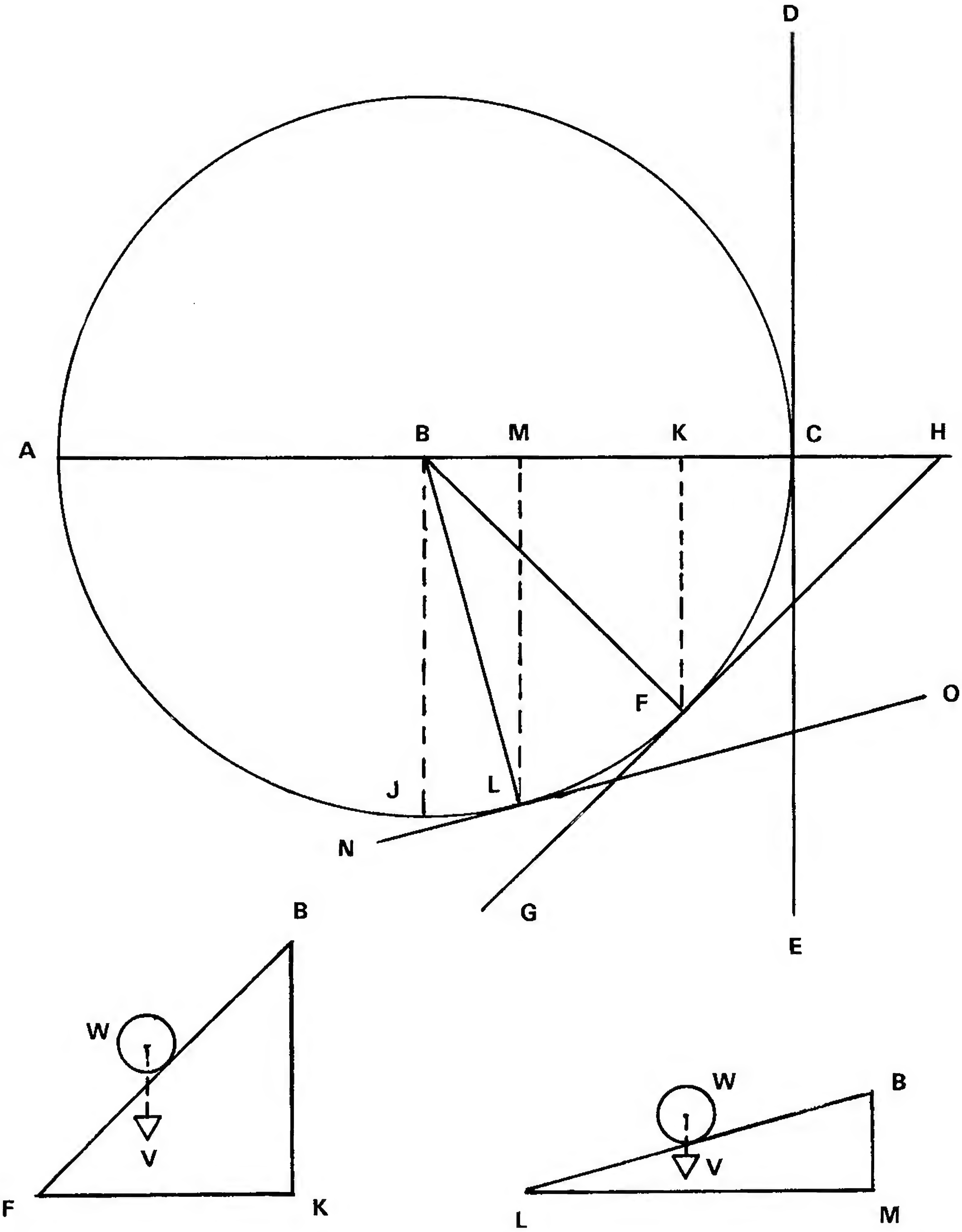
Effect: There is an observable increase in the speed of natural falling motion toward the end of the motion.

Cause: The falling body is less heavy at the beginning of its motion than it is at its end.

Intermediate stage: Supposition: heavy bodies move downward by reason of their weight (*gravitas*), and thus their speed of fall is directly proportional to their weights (GG1:262). The explanations offered by Aristotle and others invoke only accidental causes and do not arrive at the essential cause of the acceleration (GG1:317). That is, the weight of the body does not increase as it approaches its proper place; the body is not pushed by the medium rushing in behind it to fill the void created by its motion, since it is only accidental that it moves in a plenum; nor does the body encounter less resistance by having to separate fewer parts of the medium as it approaches the end of its motion (GG1:316–317).

Rather, the natural and intrinsic weight (*naturalis et intrinseca gravitas*) of the body remains constant. Thus, it is necessary to find some external force (*vis extrinseca*) that lightens the body at the beginning of its fall. This can only be the impelling force (*virtus impellens*) or lightness that sustains the body before it begins to fall and continually diminishes throughout its fall.

Such an impelling force is found not only when bodies are thrown upward before their descent, but also in cases where natural fall is not preceded by such a forced motion (GG1:318–320).



V/W = BK/BF, where BF = BA

V/W = BM/BL, where BL = BA

Figure 1. Galileo's geometrical analysis of the inclined plane experiment

Second progression:

> Cause: The effective weight of the body continually increases as this impelling force weakens and acts less against the body's essential weight.
>
> Effect: The body moves faster and faster throughout its fall from beginning to end (GG1:319).

As opposed to Aristotle's cause, Galileo feels that he has discovered the *vera causa* of velocity increase, namely, the decrease in the body's weight at the beginning of its fall. Note here the supposition on which the argument is based: Aristotle's dynamic law, $V \propto W$, which Galileo has already admitted cannot be verified by *periculum* or experiment. Galileo bases his explanation on an upwardly directed *impetus* or "levity" impressed on the body that is self-expending with time. At the summit of the object's upward motion, that *impetus* imparted to it exactly balances the body's natural *gravitas* and so the body comes to vertical rest. Then, as the *impetus* continues to decrease, the body gets heavier and heavier and so it increases its speed of fall, finally reaching what we would call a "terminal velocity" (GG1:317–320). This final state is not observed, it was thought in Galileo's time, for in the ordinary case bodies do not fall far enough for the terminal state to be achieved.

A final *periculum* or experiment to which Galileo makes reference in the early manuscript on motion occurs in chapter 22 of *De motu,* where he speaks of dropping objects from "a high tower" (GG2:333–337; Drabkin and Drake 1960, pp. 106–110). Since Galileo was at Pisa when he wrote this, he probably performed these tests from the Leaning Tower, as his student Vincenzo Viviani later reported (GG19:606.210–218). One of Galileo's professors at Pisa, Girolamo Borro, had taught that, when two equal bodies of lead and wood are thrown simultaneously from a window, the lighter body invariably reaches the ground before the heavier one. Borro maintained that he had established this by a public *experimentum* (Borro 1575, p. 215). Galileo contested this result. While conceding that the lighter body moves more swiftly at the beginning of its motion, Galileo argued that the heavier body quickly overtakes it and reaches the ground far ahead. Thus he wrote:

> [Borro holds] that air is heavy in its own region, from which it follows that things which have more air are heavier in the region of air—and this is also Aristotle's opinion. Thus a wooden sphere, for example, since it has more air in it than a leaden one, has three heavy elements, air, water, and earth; while the leaden one, since it has less air in it, has, as it were, only two heavy elements: the result is that the wooden sphere falls [in air] more swiftly than the leaden. . . .
>
> But experience [*experientia*] shows the opposite. For it is true that wood moves more swiftly than lead in the beginning of its motion; but a little later the motion of the lead is so accelerated that it leaves the wood behind it. And if they are both let fall from a high tower, the lead moves far out in front. This is something I have often tested [*De hoc saepe periculum feci*]. Therefore we must try to find a firmer cause on the basis of firmer hypotheses. (GG1:333–334)

Galileo then argued that the lighter body cannot conserve its upward impetus as well as the heavy body, and thus it falls quickly at first, but the heavy body soon overcomes its upward impetus and so catches up with, and then passes, the lighter body.

As Thomas Settle (1983) has shown, if Galileo did test this from the Leaning Tower, there is a simple explanation of the phenomenon he observed. The tower contains seven stories that are reached from an interior stairway. If one leans somewhat to drop the objects with hands extended over the ledge, and does so at successively higher stories, one finds that at the lowest stories the wood reaches the ground before the lead, whereas at the higher stories the lead arrives well ahead of the wood. The experiment actually has been duplicated and confirms Galileo's finding. Settle and others speculate that the heavier object induces arm fatigue in one holding it out over the ledge, and this causes the holder to have a slower release or to pull up on the heavier object, thus delaying its initial fall.

All of the foregoing materials pertain to the 1590 period. Galileo had wanted to publish the treatise on motion, but he had doubts about the "true causes" he had proposed in it because of his failure to obtain experimental confirmation of his results. He kept the manuscript in his possession, nonetheless, and when he finally did discover the correct law of falling bodies, as I shall show later, he inserted a draft of his discovery among the folios of the manuscript, thus signaling its role in the discovery process (Fredette 1972; Camerota 1992).

Novelties in the Heavens

We move now to the next period of experimental activity, mainly at Padua and roughly from 1604 to 1612, during which Galileo also made his important discoveries with the telescope. Here again he imported mathematical techniques into the demonstrative regress and thus was working in the "mixed" or "middle science" (*scientia media*) tradition. For the work with the telescope with which he revealed his "novelties in the heavens," Galileo relied mainly on projective geometry, whereas with his more advanced studies of the kinematics of motion he had to investigate properties of conic sections, particularly those of the parabola. Since the uses of projective geometry are simpler, I begin with them and later consider the more complicated experiments of his kinematical researches, even though this reverses the chronological order of their performance.

Precisely how the regress works in astronomy may be seen from a study of Galileo's treatise on the sphere, the *Trattato della sfera ovvero Cosmografia,* which he composed at Padua around 1602. The context is his explanation in the *Trattato* of the aspects and phases of the moon and the ways these vary with the moon's synoptic and sidereal periods (GG2:251–253). These phenomena depend only on relative positions within the earth-moon and earth-sun systems and do not require commitment to either geocentrism or heliocentrism, being equally well explained in either. Basic to the explanation is the conviction that these aspects and phases are effects (*effetti*) for which it is possible to assign the cause (*la causa;* GG2:250). Among the causes Galileo enumerates are that the moon is spherical in shape, that it is not luminous by nature but receives its light from the sun, and that the orientations of the two with respect to the earth are what cause the various aspects and the places and times of their appearances. The argument follows closely the paradigm provided by Aristotle in *Posterior Analytics* (I.13) to show that the moon is a sphere. It involves only one supposition, that

light travels in straight lines, and this is what governs the intermediate stage. This allows one to use projective geometry to establish the convertibility condition, namely, that *only* external illumination falling on a shape that is spherical will cause the moon to exhibit the phases it does at precise positions and times observable from the earth. The reasoning may be summarized as follows (from Wallace 1992a, pp. 194–197):

First progression:

Effect: The moon's aspects and phases.

Cause: Its spherical shape, illumined by the sun, at various positions and times.

Intermediate stage: The moon is not luminous by nature; it is externally illumined by the sun, and it is observed from many different angles; *only* a shape that is spherical and this illumination will, under these circumstances, exhibit the aspects and phases it does at precise positions and times observable from the earth. The precise phenomena can be calculated from the supposition (*ex suppositione*) that light travels in straight lines, using theorems proved in projective geometry.

Second progression:

Cause: The moon's spherical shape, illumined by the sun, at various positions and times.

Effect: The moon's aspects and phases, calculated using the laws of geometrical optics.

When Galileo made his exciting discoveries with the telescope in 1609–1610 this same paradigm was ready at hand for further exploitation. Others before him had constructed telescopes, and some had even looked at the heavens with them, but none would formulate the "necessary demonstrations" Galileo would propose on the basis of his observations. We know that between November 30 and December 18 of 1609 Galileo studied the moon with his new instrument and made no fewer than eight drawings of the appearances he observed. On January 7, 1610, he wrote to Antonio de'Medici in Florence that, from the data he had obtained, "sane reasoning cannot conclude otherwise" than that the moon's surface contains mountains and valleys similar to, but larger than, those spread over the surface of the earth (GG10:273). Thus, within about a month, by his own account, Galileo had demonstrated to his personal satisfaction that there are mountains on the moon.

The regress that supports this reasoning may be schematized as follows (from Wallace 1992a, pp. 198–201):

First progression:

Effect: Sharply defined spots on illuminated parts of the moon's surface, an irregular line at the terminator, with points of light emerging in the dark parts.

Cause: The surface of the moon is rough and uneven, with bulges and depressions (GG3.1:62–63).

Intermediate stage: Dark part of spots have their side toward the sun; shadows diminish as the sun climbs higher; points of light in the dark area gradually increase in brightness and size, finally connect with the dark area; "we are driven to conclude by necessity" that *only* prominences and depressions can explain the appearances "for certain and beyond doubt" (GG3.1:64–69).

Second progression:

Cause: Changing illumination from the sun's rays on mountains of calculable height rising from the moon's surface.

Effect: All of the observed appearances (GG3.1:69–70).

Here again there are the two progressions, the first *quia* from effect to cause, the second *propter quid* from cause to effect, with the intermediate stage establishing the convertibility condition and thus the connection between the two. The implied supposition, not indicated here, is the same as that underlying the *Trattato della sfera* demonstrations, namely, that light travels in straight lines. Those who see this and carefully observe the phenomena, wrote Galileo, "are driven to conclude by necessity" that *only* prominences and depressions on the moon's surface can explain its appearances "for certain and beyond doubt" (GG3.1.64–69; Galileo 1989, pp. 39–48).

On the very evening in which Galileo wrote to Antonio de'Medici that he had demonstrated the existence of mountains on the moon, he noted a further strange phenomenon, namely, that the planet Jupiter was "accompanied by three fixed stars" (GG10:277). That was on January 7, 1610. The next night Galileo turned his telescope on the heavens again, hoping to see that Jupiter had moved to the west of these stars, as Ptolemaic computations then predicted (GG3.1.80). To his surprise this time he found the planet to be east of them. His attempt to resolve that anomaly led him to a program of observing Jupiter and its strange companions whenever he could over a two-month period. By January 11 he had concluded that they were not fixed stars that could be used to determine the motion of Jupiter, but rather were small bodies, never observed before, that were moving along with Jupiter and indeed were actually circling it. "I therefore arrived at the conclusion, entirely beyond doubt [*omnique procul dubio*]," he wrote, "that in the heavens there are three stars wandering about Jupiter like Venus and Mercury around the sun" (GG3.1.81). On January 13 he saw a fourth object for the first time, and by the 15th he had convinced himself that it was doing the same (GG3.1.82). So within a week of his curiosity having been aroused by the anomaly, he had completed the demonstrative regress and had convinced himself that Jupiter has four satellites revolving about it, as it made its own majestic revolution around the center of the universe (GG3.1.80–95; Galileo 1989, pp. 64–84).

The reasoning process Galileo employed in this discovery may be outlined as follows (from Wallace 1992a, pp. 201–203):

First progression:

Effect: Four little stars accompany Jupiter, always in a straight line with it, and move along the line with respect to each other and to Jupiter.

Cause: The stars are planets of Jupiter, circling around it at various periods and distances from it.

Intermediate stage: Sixty-five observations between January 7 and March 2, analyzing in detail their variations in position, how they separate off from Jupiter and each other and merge with them in successive observations; inference to the *only* possible motion that explains these details; concluding "no one can doubt" (*nemini dubium esse potest*) that they complete revolutions around Jupiter in the plane of the ecliptic, each at a fixed radius and with its characteristic time of revolution (GG3.1.94).

Second progression:

Cause: Four satellites of Jupiter always accompany it, in direct and retrograde motion, with their own distances from it and periods of revolution (GG4:210), as it revolves around the center in twelve years.

Effect: Seen on edge, the satellites produce the appearance of four points of light, moving back and forth on a line with the planet and parallel to the ecliptic.

The basic supposition is again that light travels in straight lines. Over and above that, of course, one has to know enough projective geometry to recognize that satellites circling around an equatorial plane, when seen on edge, would appear to be moving back and forth on a line parallel to the planet's equator and along the elliptic. Galileo quickly saw the convertibility of the geometry involved, going from the straight-line motion he actually observed to the circular motion that alone could cause it, and then regressing from the cause back to the effects he had so carefully observed.

Space does not permit me to exhibit the completely analogous reasoning process by which Galileo, in December of 1610, having by then observed the phases of Venus, could demonstrate that the planet is in orbit around the sun. The geometry in this case is considerably more complex than that required to complete the intermediate stages outlined in the previous examples of the regress. But when one understands the geometry involved, it is a simple matter to understand why, when seen from the earth, Venus exhibits the phases it does and its changes in size and appearance. One can also see why there is no possibility that Venus could be rotating around the earth, but *must* be orbiting the sun (Wallace 1992a, pp. 203–207). It is this demonstration, along with the previous ones, that Galileo clearly had in mind when in 1615, in his famous *Letter to the Grand Duchess Christina,* he wrote so glowingly about his "necessary demonstrations based on sensible experience." It is perhaps significant that he uses this expression or its equivalent over forty times in that much-quoted letter (Moss 1986).

The "Tabletop" Experiments

These astronomical discoveries, of course, are truly wonderful demonstrations, and one can readily understand why, as their significance was grasped, they brought Galileo almost immediate fame throughout Europe. And yet in the final analysis they are not as important as the series of experiments on motion and falling bodies he performed at Padua immediately prior to the telescopic discoveries in the years 1604–1609. In these tests, known as the "table-top" experiments, Galileo used an inclined plane placed on the edge of a table to establish (1) the correct speed law, that velocity is proportional not to the distance of fall, as he earlier thought, but to the square root of distance; (2) the correct distance law, that distance of fall is proportional to the square of the time of fall; and (3) that the path a body follows when projected horizontally at uniform velocity and then allowed to fall under the influence of gravity is a semi-parabola. All of these results were established by Galileo through the use of the demonstrative regress, as I will now explain.

Around 1602, while in correspondence with Guidobaldo del Monte, Galileo experimented with the pendulum as an alternative to the inclined plane, because, although the bob of the pendulum moves along the arc of a circle rather than a chord, it eliminates the surface friction always present on the plane (Naylor 1974). Galileo had already rejected the Aristotelian dynamic law, that speed of fall is uniform and simply proportional to weight. In 1604 he wrote to Paolo Sarpi stating that speed increases with distance of fall, and from this principle he was trying to deduce various properties of falling motion (GG10:115–116). Shortly after that he apparently initiated experiments with an inclined plane situated on the top of a table with its base at or near the table's edge, thus allowing a ball to roll down the incline and then drop freely to the floor. These experiments were totally unknown until about 1972, when Stillman Drake uncovered folios in MS Gal. 72 that gave evidence of them (Drake 1973, 1978). Since then they have been analyzed in detail and duplicated by Drake, Ronald Naylor (1976, 1980, 1990), and David Hill (1979, 1986, 1988). Collectively their results show that Galileo was engaged in a serious research program in the first decade of the seventeenth century, achieving an experimental accuracy within three percent when testing his calculated results.

As shown in figure 6.2, this program made use of four different, but connected, types of experiment, probably made in the progression as shown from top to bottom. The first type, shown in figure 6.2a, was designed to ascertain the correct speed law, to show that distance of horizontal projection after various distances of roll down the incline do *not* vary as the distance of roll but rather as the square root of that distance.[3] With this knowledge in hand, Galileo then began to work on defining the characteristics of the curves that result when the angle of inclination is varied. This is the second type of experiment, fairly complex, shown in figure 6.2b. The curves shown there approach more and more a semi-parabolic form the smaller the angle of incline, suggesting that a straight horizontal projection might yield the semi-parabola.[4] The problem then became one of achieving such projection while at the same time having a way to vary and measure the ball's velocity on leaving the tabletop. Galileo's solution is shown in figure 6.2c, illustrating his design of different deflectors to produce

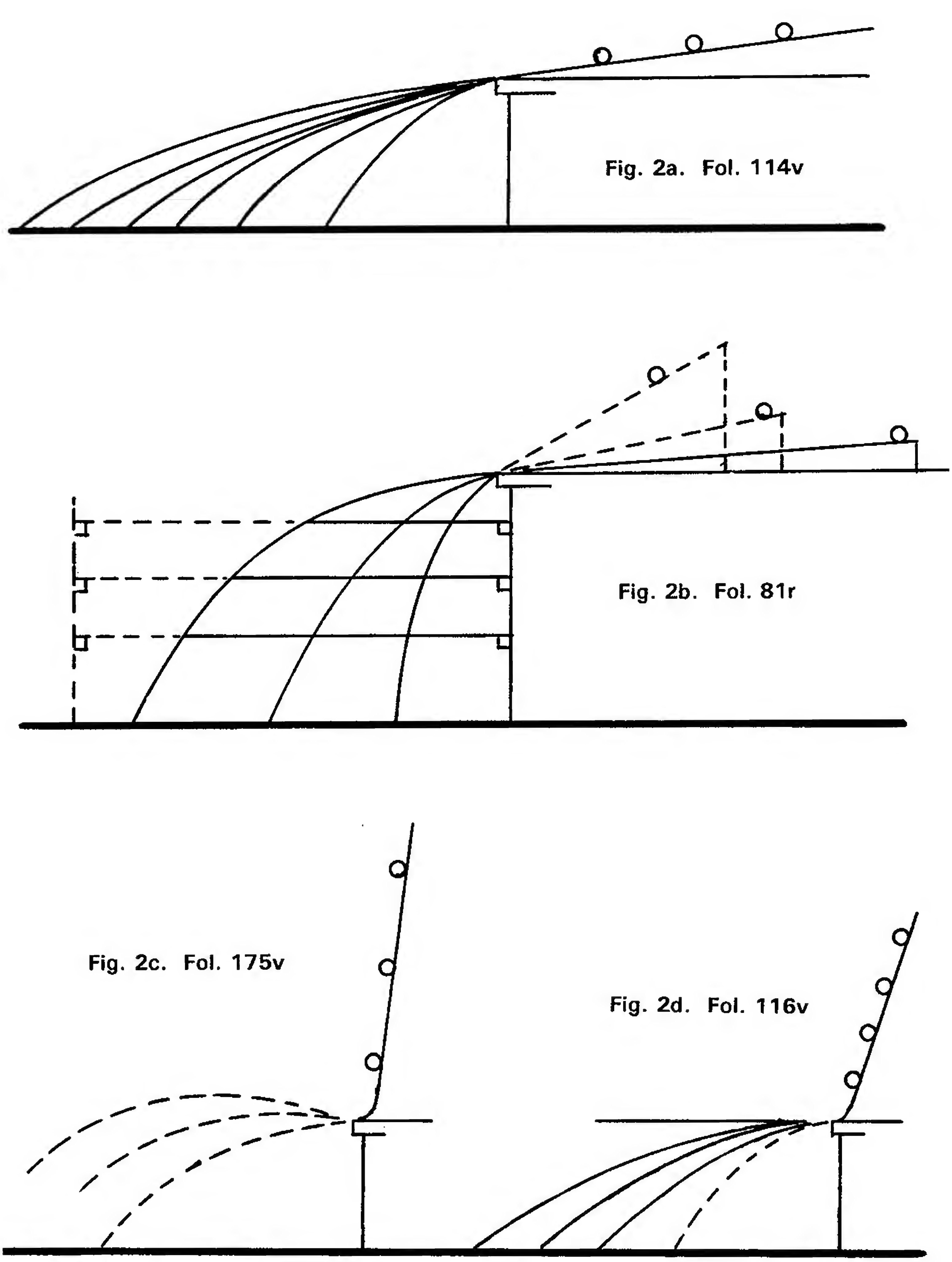


Figure 2. Galileo's Table-top Experiments, MS Gal. 72, Padua 1604–1609

a variety of curves,[5] and in figure 6.2d, the one he finally used to achieve horizontal projection, along with the series of semi-parabolic curves he eventually produced.[6]

The key result that emerges from these experiments is that the speed of bodies in free fall, instantiated by balls that are no longer on the incline but have left it and are falling naturally, varies directly as their time of fall. From this principle, explicitly stated at the beginning of the Third Day of the discourses of the *Two New Sciences,* Galileo derives most of the propositions he presents in the Third and Fourth Days of that work (Wallace 1992a, pp. 287–289). His reasoning in establishing that principle may be abbreviated as follows:

First progression:

Effect: The various properties of heavy bodies moving with a motion that is naturally accelerated.

Cause: Their falling at a speed is directly proportional to their time of fall.

Intermediate stage: This is proved kinematically, because only a falling speed directly proportional to the time of fall can produce distances that satisfy the odd-number rule and the times-squared rule in vertical fall, the double-distance rule when the vertical speed is converted to horizontal speed, and the semi-parabolic path when free fall occurs after the vertical speed has been converted to horizontal speed—by geometrical demonstration, from the supposition (*ex suppositione*) that all impediments such as friction, the resistance of the medium, and all other accidental factors have been removed.

It is also argued from physical considerations: for nature itself (*instituti ipsiusmet naturae*) causes the falling motion of a heavy body, which is a natural motion, to increase in the simplest way: by adding equal increments to the speed in equal intervals of time. It is also argued from disproof of the simplest alternative, since speed does not increase directly with the distance of fall but rather with the square root of that distance.

It is confirmed experimentally, for physical experiments (*naturalia experimenta*) show that all these metrical properties are verified within degrees of accuracy that allow for slight departures owing to impediments and accidental causes (GG2:261, 8:197).

Second progression:

Cause: A heavy body that is naturally accelerated in free fall at a speed that is directly proportional to its time of fall from rest.

Effect: Metrical properties described by the odd-number, times-squared, and double-distance rules and by paths of semi-parabolic projection.

As can be seen here, the regress is employed once again to arrive at the true cause, what becomes for Galileo the definition of naturally accelerated motion. The first progression is *a posteriori,* from effect to cause, and the second *a priori,* from cause to

effect. The intermediate stage, the work of the intellect, carries the burden of proof, as heretofore. Actually its wording as shown here follows closely Galileo's Latin text in his draft of this passage, the *De motu accelerato* fragment now bound in MS Gal. 71 (GG2:226), where Galileo inserted it after writing it out. It also appears in the *Two New Sciences,* and with almost identical wording (GG8:198).

The demonstration here, like the earlier ones, is explicitly made *ex suppositione,* that is, on the supposition that all impediments to the falling motion, such as friction, resistance of the medium, and accidental factors, have been removed. The proof is based partly on the elimination of the simplest alternative, that speed of fall is based on distance of fall, as Galileo himself had first thought. But the direct proof is experimental. Note the reference to "physical experiments," pointedly in the plural. The reference is not to the simple inclined-plane experiment described in the *Two New Sciences,* as it has commonly been taken, but to the whole gamut of experiments, tabletop included, performed at Padua before the discoveries with the telescope. Note further that Galileo no longer identifies the weight of the falling body as the cause of its fall, as in his early formulations. Now he is interested solely in the kinematic factors that bear on the quantitative aspects of naturally accelerated motion. As for the ultimate physical cause of the fall, he identifies this simply as "nature," the ultimate explanatory principle in Aristotelian physics. So he himself is working unambiguously in the tradition of a mathematical physics, a "mixed" or "middle science." What he proposes to do for dynamics is what Archimedes has done for statics, that is, provide a hitherto unknown science of local motion based on mathematics and not on physical principles alone (Wallace 1984, pp. 272–276; 1992a, pp. 270–273, 285–293).

Mathematical Physics

Galileo, like Newton after him, thus regarded himself as a mathematical physicist. It is interesting to see where he conceived his own genius to lie in working out the demonstrations he offered, proofs that would characterize his "new science." In my view his ability lay in knowing how to pose suppositions that permit experiments to be made, and then verifying, in the experiments themselves, that the suppositions hold up within the degree of accuracy required to justify them. Galileo himself hints at this in a passage of the Second Day of the *Dialogue on the Two World Systems* of 1632, where Simplicio and Salviati are discussing whether a sphere touches a plane at a point (GG7: 233–234). Here Simplicio is arguing, in effect, that mathematics cannot be used in physics because abstract spheres are not the same as material spheres and this difference vitiates Galileo's calculations. That is not Aristotle's argument, though it was used by Peripatetics of Galileo's day. Aristotle himself acknowledged the validity of "mixed sciences," those that use mathematics to establish conclusions in physics, and indeed used them, as I have shown, to exemplify the demonstrative regress. So Galileo's reply to Simplicio's argument is authentically Aristotelian, being based on the materials in the logical treatises of his MS Gal. 27 (Wallace 1981; 1992a, pp. 139–149):

> I readily grant you all these things, but they are beside the point. . . . By your own statement, spheres and planes are either not to be found in the world, or if they found they are spoiled upon being used for this effect. It would therefore have been better for you

> to grant the conclusion conditionally [*condizionatamente*]; that is, for you to have said that if there were given a material sphere and plane that were so perfect and remained so, they would touch one another in a single point, and then to have denied that such were to be had. . . .
>
> Whenever you apply a material sphere to a material plane in the concrete, you apply a sphere which is not perfect to a plane which is not perfect, and you say that these do not touch each other in one point. But I tell you that even in the abstract, an immaterial sphere which is not a perfect sphere can touch an immaterial plane which is not perfectly flat in not one point, but over a part of its surface, so that what happens in the concrete up to this point happens in the same way in the abstract. . . .
>
> Do you know what does happen, Simplicio? Just as the computer who wants his calculations to deal with sugar, silk, and wool must discount the boxes, bales, and other packings, so the geometrical physicist [*filosofo geometra,* Galileo's term for the mathematical physicist of his day], when he wants to recognize in the concrete the effects which he has demonstrated in the abstract, must deduct the impediments of the matter [*gli impedimenti della materia*], and, if he is able to do so, I assure you that his results are in no less agreement than arithmetical computations. *The errors, then, lie not in the abstractness or concreteness, not in geometry or in physics, but in the calculator who does not know how to make an accurate calculation.* Hence if you had a perfect sphere and a perfect plane, even though they were material, you would have no doubt that they touched in one point. . . . (GG7:232–234)

As Galileo explains, one must understand the differences between abstract spheres and material spheres, and know how to "deduct the impediments of the matter," if one is to make accurate calculations in physics. To do so the mathematical physicist must proceed "conditionally" in his discipline. Galileo's term is *condizionatamente,* which has the same meaning as *ex conditione* or *ex suppositione.* The condition or supposition is that the scientist recognize these impediments and devise experiments that can circumvent them. If he can do so, his results will be true within the limits he has set for himself on the basis of his own suppositions. The calculator who is unable to do this, who does not understand the mathematics or who is unable to perform experiments of this type, will always get erroneous results. But then the fault is not in the mathematical physics but rather in the experimenter who lacks the expertise to verify his insights.

Galileo the Controversialist

Although Galileo has a well deserved reputation as a polemicist and controversialist, his scientific writings to this point were not essentially polemical. His early work on motion at Pisa did develop out of disputations at the university in which Galileo upheld a progressive view, and undoubtedly he was concerned that his arguments would be rejected by conservative Aristotelians, including his teacher Francesco Buonamici (GG1:398.4–19, 412.19–22). Still, his ideas mirrored in some ways those already advanced by Giovanni Battista Benedetti and others (Wallace 1987). The important point is that they were never published, and this alone would explain why they did not spark any controversies. His discoveries with the telescope, of course, touched on a very controversial subject, the nature of the heavens, and yet his manner of reporting them

in the *Sidereus Nuncius* gave his adversaries little ground for rejecting them. The main problem they posed was their factual status, for those not having access to a telescope with sufficient magnification and resolving power would be tempted to dismiss the phenomena he reported as optical illusions. Albert Van Helden has shown that astronomers who might have been expected to reject them on philosophical grounds, such as the Jesuit professors at the Collegio Romano, actually verified Galileo's findings as soon as they had constructed a good telescope themselves (Galilei 1989, pp. 110–112). This situation seems quite representative, for despite some early opposition from irresponsible authors such as Martin Horky, Francesco Sizzi, and Giulio Cesare Lagalla, Galileo's findings were soon accepted without argument by astronomers throughout Europe.

A similar situation obtains with regard to the tabletop experiments performed by Galileo at Padua in the first decade of the seventeenth century. Like the materials present in MSS Gal. 27, 46, and 71, none of the findings recorded in MS Gal. 72 was known in Galileo's day. They stimulate controversy in our day over how they are to be interpreted, but that is properly a problem for historians and not for scientists—unless one is to consider the possibility mentioned by Gideon Freudenthal of a scientist having a controversy with himself. In that event the older Galileo was arguing with the younger, and what he was contesting was simply the unproved suppositions on which his earlier "demonstrations" had been based.

Shortly after 1610, however, Galileo himself became deeply involved in controversy, and unfortunately this state continued more or less uninterruptedly until the end of his life. Many of these controversies were more theological than they were scientific, being concerned with how the scriptures were to be interpreted and what latitude should be allowed to those who departed from the traditional teachings of the Church. As to the scientific problems with which he had then to deal, most of these were not solvable with the information available to Galileo or anyone else, and so were not amenable to the use of the demonstrative regress. Much of Galileo's part in them, however, can be understood in terms of various adaptations he seems to have made when applying regressive methods to situations where certitude could not be attained and one had to resort to probable reasoning. Three cases that illustrate these adaptations, which invoke a type of dialectical (as opposed to demonstrative) regress, turn out to be representative of his future work. All three took place in the second decade of the seventeenth century, shortly after his remarkable success with the tabletop experiments and his discoveries with the telescope. The first two involved actual controversies: Galileo's dispute with Ludovico delle Colombe at Florence in 1612 over the true cause of flotation, and his prolonged debate with Christopher Scheiner in 1618 over the nature of sunspots. The third controversy occurred in the interval between the other two and was not itself a controversy, although it was to give rise to one a decade and a half later. This was his proposal to Cardinal Orsini in 1616 sketching his argument from the tides to prove the earth's motion, which was to have disastrous consequences for Galileo when he reformulated it in the *Dialogue* of 1632.

Galileo's preferred technique in controversy was to set up two mutually exclusive or dichotomous explanations for a particular phenomenon and then devise various observational or experimental texts that would serve to eliminate the one and thus leave the other. When coupled with geometrical methods of proof, this technique

lends itself to a *reductio ad impossibile* for one of the alternatives and thus supplies indirect proof for the other. The dichotomy itself functions as a *suppositio* in the proof, and is particularly effective if it is proposed by, or is acceptable to, the other party to the controversy.

In the dispute with Colombe the supposition was that a body's motion downward in a medium was caused either by the shape of the body (Colombe's alternative) or by the weight of the body in the medium in which it is placed (Galileo's alternative). The argument Galileo proposed in support of his side is based on hydrostatic principles, properly applied through geometrical analysis, to make clear the proper cause of flotation. The conclusion to which it came is that the true, intrinsic, and proper cause of flotation and submergence, excluding mediate and accidental causes, is the weight of a body relative to that of the medium. That is to say, it is not the body's absolute weight or *gravitas* that determines whether the body will float or not, but rather its *propria gravitas,* its weight in the medium in which it is immersed, considering that the body is buoyed up by a force equal to the weight of the fluid it is able to displace. For Galileo, this alone explains why one body will float in a medium and others will not, how much will protrude above the surface when it does, and how a medium can support a weight heavier than itself (GG4:79).

To meet Colombe's counterarguments, Galileo admits that a body's shape may affect the speed of its motion through a medium, but this is not the proper cause of its motion. This can be demonstrated by experimenting with a mass of wax molded into various shapes; its position in the medium is determined by its weight and not by any particular shape it is made to assume. The special case of a thin plate of ebony floating on water can then be explained by an accidental cause. Here Galileo formulates the ingenious proposal that the volume of air enclosed by ridges and below the water's surface, when joined to the unwetted top surface of the plate, adds to the plate's buoyancy and so causes it to float (GG4:107–111). Thus he sidesteps the problem of surface tension, focusing on an equilibrium situation in which the causes that might produce motion cancel out and a simple volumetric solution can be provided using geometrical principles (Wallace 1992a, pp. 276–278).

The dispute with Scheiner over sunspots lent itself to the same technique. According to Scheiner the observed appearances of the spots may be explained in one of two ways: either as spots moving on or near the sun's surface or as spots rotating in a celestial sphere outside the sun, presumably "stars" or planets. Anxious to preserve the sun's unalterability and incorruptibility as a heavenly body, Scheiner opted for the second alternative, leaving Galileo the opportunity to exploit the first. This he did by subscribing to Scheiner's dichotomy and then attacking the latter's position, again through the use of geometrical analysis—this time using the principles of optics rather than those of hydrostatics.

Geometrical optics, Galileo states, provides necessary demonstrations that the spots are not outside the sun but are contiguous with its surface. In particular, the spots appear thinner when near the edge of the sun than when close to its center; the distances they travel increase as they approach the center and decrease as they recede toward the edge; and they separate more and more as they approach the center—for one who knows *perspettiva,* "a clear argument [*manifesto argomento*] that the sun is a globe and that the spots are close to the sun's surface" (GG5:119; Shea 1972, pp. 55–57).

Furthermore, close observation shows that the appearances of the spots are not those of stars (*stelle*); they more resemble clouds (*nugole*) that form and dissolve and so change size and shape. Thus, Galileo observes, it is not certain that the same spots return after a complete revolution, nor is it certain that the sun itself rotates on its axis, although it appears to do so (GG5:133).

It is interesting to note that, although Galileo claims to incorporate "necessary demonstrations" (*dimostrazioni necessarie*) in his overall argument, and so is successful in negating Scheiner's position, he himself advances only probable opinion as to what the spots ultimately might be. Thus his conclusions may be summarized as follows: the spots are definitely not stars or planets rotating in their own celestial orbits around the sun somewhere between it and earth; it is probable that they are clouds in a medium surrounding the sun's surface; and it is more probable that the sun itself rotates and carries this medium and its clouds along with it than that these have an independent circular motion around the sun (Wallace 1992a, pp. 207–211).

The third case I examine does not invoke a dichotomy as does the first two but employs causal argument to assign degrees of probability to various possible explanations for a given phenomenon, along lines already seen in the dispute with Scheiner. In this and similar cases Galileo's various causal maxims assume importance, namely, that there is only one true and primary cause for any one effect; that effects similar in kind must be reducible to a single true and primary cause; that there is a fixed and constant connection between cause and effect, so that any alteration in the one will be accompanied by a fixed and constant alteration in the other, and so on (Mertz 1980; Wallace 1983, pp. 612, 622). In this case the effect to be explained is the ebb and flow of the tides in the various oceans and seas on the earth's surface, which Galileo suspects might be connected with the motion of the earth. In his *Letter to the Grand Duchess Christina* of 1615 Galileo had made reference to "physical effects whose causes perhaps cannot be determined in any other way" (GG5:311) without indicating precisely what he had in mind. Apparently he discussed this with a young friend, Alessandro Orsini, who had just been made a cardinal and who asked Galileo to write out his argument. Galileo did so on January 8, 1616, in a letter now entitled *Discourse on the Tides* (GG5:377–395).

Galileo begins by noting that sensory appearances show that the tides involve a true local motion in the sea, and thus to find their cause one must investigate the various ways motion can be imparted to water. He further notes the complexity of tidal phenomena, and on this account will see if any of the possible movers can reasonably be assigned as the primary cause. To this he then proposes to add secondary or concomitant causes to account for the diversity of the tides' movements. Since the motion of the container can often explain the motion of the fluid it contains, Galileo speculates that "the cause of the tides could reside in some motion of the basins containing the seawater," thus focusing on the motion of the terrestrial globe as "more probable" than any other cause previously assigned (GG5:381). On this basis he takes the motion of the earth hypothetically (*ex hypothesi*) and, from its two motions, one of annual revolution around the sun, the other of diurnal rotation on its axis, explains how it might function as a primary cause of the back-and-forth motion of the water on its surface. This cause will obviously not be enough to account for the particular details of tidal phenomena, and so to it he adds additional causes, among them the gravity of sea-

water, the length and depth of the basin in which it is contained, the frequency of its oscillations, and the ways these might be coordinated with the movement of various parts of the earth.

Galileo concludes on the note that with this explanation he is able to harmonize the earth's motion and the tides, "taking the former as the cause of the latter, and the latter as a sign of and an argument for the former" (GG5:393). His expression here clearly signals the use of the demonstrative *regressus,* despite the fact that the argument he is proposing is just as clearly not a demonstration. To take account of both features, we propose to modify our earlier formulation of the *regressus* to accommodate it to probable argument. The revised form is the "dialectical regress" to which reference has already been made. When applied to Galileo's early statement of the tidal argument the *regressus* may be seen to proceed as follows (from Wallace 1992a, pp. 211–216):

Possible cause: from an effect to one or more hypothetical causes that might be sufficient to produce it.

Effect: The ebb and flow of the tides in various oceans and seas on the earth's surface.

Possible Cause: Primarily a twofold motion of the earth, secondarily by auxiliary factors.

Dialectical inquiry: use of probable reasoning and correlations to specify in detail the causal factors that produce the effect. The motion of a container can explain the motion of water within it; the diurnal and annual motions of the earth produce unequal motions at different parts of the earth's surface; the oscillations set up in bodies of water by these unequal motions vary in period depending on the lengths and depths of the sea basins. These unequal motions also are of two types and have two components, one vertical, seen mainly at the extremity of the basins, the other horizontal, seen mainly at their middle; in very large seas differential factors further operate to produce more movement in some parts than in others.

Tidal periods of twelve hours are produced by the primary cause; those of six, four, three, and two hours are produced additionally by various combinations of secondary causes. The motion of the moon is a fictitious cause that has nothing to do with tidal motions (GG5:381–393).

Probable cause: from one or more causes now regarded as probable to the actual production of the effect.

Probable Cause: Twofold motion of the earth, acting on bodies of water of different shapes and sizes.

Effect: An ebb and flow of tides at characteristic periods in the respective basins.

Note that there is no air of controversy in this initial presentation of the tidal argument. It was written, as already observed, after Galileo's letter to Christina. It was

written also *after* Cardinal Bellarmine's letter to Foscarini (and Galileo) warning against using the earth's motion, without offering demonstrative proof, to question the Church's traditional interpretation of Scripture, and *before* the Church's decree against teaching or defending Copernicanism, which was dated March 5, 1616. Thus it reflects Galileo's thought on the tidal proof at a relatively tranquil period in his life—well before he got embroiled in the bitter controversies over scriptural interpretation that would lead ultimately to his trial and condemnation by the Church in 1633.

The subsequent history of the "Galileo Affair" has been rehearsed so many times that it does not require repetition here (Finocchiaro 1989). Suffice it to mention that Galileo was a skilled controversialist, and he did not fail to use all of the means of dialectics and rhetoric to argue the case for the earth's motion (Moss 1983, 1986, 1993; Finocchiaro 1980). On the other hand, his expressed intention was only to make that case "persuasible" (*persuasibile,* GG7:30.22), and not once did he ever claim to have demonstrated the earth's motion as an epistemic conclusion. Unfortunately, this fact seems often overlooked in the vast literature that now surrounds the infamous affair.

Regarding the various views of scientific controversies proposed at this conference, most can be seen as verified in one way or another in the work of Galileo. Some analyses apply more readily to recent science than they do to that of the early modern period, and for this reason, as for reasons of brevity, I shall be selective in my comments. Following Philip Kitcher's taxonomy (see chapter 1 in this volume), I would say that Galileo subscribed to a rationalistic model of controversy wherein the issues argued would have testable consequences and where epistemic closure would be sought through necessary arguments or ones that engender highly probable conclusions. In his dialectics Galileo was particularly expert at blocking his opponents' lines of escape. In those cases where he could offer demonstrations on the model of Aristotle's *Posterior Analytics,* my impression is that he won over his adversaries rather quickly considering the novelty of the conclusions to which he had come. Similarly, employing Aristides Baltas's taxonomy (see chapter 2 in this volume), Galileo again followed a rationalistic model. His "constitutive assumptions" were generally unexpressed but they are clearly those of Aristotelian and commonsense realism combined with those of Euclidean geometry. His "controversial assumptions," on the other hand, were explicitly recognized and were generally formulated as suppositions (*suppositiones*) that, in his physics, would have argumentative force similar to that of postulates or petitions in classical mathematics.

Marcello Pera's analysis (see chapter 3 in this volume) comes closer to Galileo's timeframe and thus is more applicable to my account. Galileo's epistemology was still premodern, and his discussion was dialectical, largely internal and epistemic. He definitely proposed some of the premises of his arguments as propositions to be agreed upon, and these again he labeled "suppositions." His logic was clearly both a posteriori (or inductive) and a priori (or deductive), each constituting a different phase or progression in the demonstrative regress, and it aimed for both formal and content validity. Rhetoric and dialectic were frequently intermingled in Galileo's discourse, particularly in his prolonged crusade in support of the Copernican opinion, although Pera's view of the respective spheres of rhetoric and dialectic is somewhat at variance with my own (Wallace 1992a, pp. 128–130).

A similar appraisal might be made of Peter Machamer's account (chapter 6), which agrees in most particulars with the analysis presented in this chapter. Additional points of agreement may be noted in two of the remaining contributions. With regard to Gideon Freudenthal's analysis (chapter 7), all of Galileo's discoveries had cognitive content, no previously available solutions were at hand, and vital cosmic and religious interests were at stake. Strictly speaking, no new system of physics was yet in question, since Galileo's methods mainly involved adjustments within an Aristotelian-Archimedean-Euclidean framework. And respecting Mauritio Mamiani's canons (see chapter 8), disputes were generally at the levels of new observations or experiments, all involving matters of fact. There was little theory in the modern sense, and any unobservables that might have been involved were entities or measurements that previously had escaped observation in ordinary sense experience.

Finally, when the chapters are considered as a whole it becomes apparent that, as a controversialist, Galileo the scientist had few equals. But the main lesson I have been urging in this chapter is somewhat different. Put simply, the best way to end a scientific controversy is to produce a convincing demonstration. I readily grant that such demonstrations are difficult to come by when one is working at the frontiers of knowledge. But Galileo faced that situation, too, and he succeeded where all others since Aristotle had failed. That is why we rightly honor him as the Father of Modern Science.[7]

Notes

1. Antonio Favaro, ed., *Le Opere di Galileo Galilei,* 20 vols. in 21 (Florence, 1890–1909, repr. 1968), 9: 273–282 [cited hereafter as GG].

2. Averroes was a rationalist in his understanding of Aristotle and saw no role for mathematics in the study of nature. Aquinas, on the other hand, was more the empiricist, as was his teacher Albertus Magnus. He also developed Aristotle's teaching on the mixed sciences, which he called "middle sciences" or *scientiae mediae* because they are intermediate between mathematics and physics, and used them as a model for the science of revealed theology. See Aquinas's *Summa theologiae,* Part I, Quest. 1, Art. 2.

3. This result is based on Hill's (1988) interpretation of a diagram found on fol. 114v of MS Gal. 72 (pp. 658–659). Galileo lists a series of numbers for different lengths of horizontal projection along the floor, namely, 253, 337, 395, 451, 495, 534, and 573. Hill, in attempting to duplicate Galileo's figures, has found that increasingly longer lengths of roll down an inclined plane inclined at an angle of 12° to the table top, with the lengths standing in the ratio of 1:2:3:4:5:6:7, will yield Galileo's figures approximately. If one takes the starting length of roll at 400, the successive lengths will be 800, 1200, 1600, 2000, 2400, and 2800. Taking the square root of the middle figure in this sequence, 1600, and fitting it to the middle figure in the horizontal projections, 451, one obtains a sequence very similar to Galileo's, namely, 226, 319, 390, 451, 505, 552, and 596. This would seem to confirm that the distance of horizontal projection, which is a measure of the ball's velocity on leaving the incline, is as the square root of the length of roll down the incline. Arguing *a pari,* this would seem to show that velocity of fall is *not* proportional to distance of fall, as Galileo had conjectured in his letter to Sarpi, but rather is proportional to the square root of that distance.

4. The diagram here is based on a figure drawn by Galileo on fol. 81r of MS Gal. 72. This has been analyzed in various ways by Naylor and Hill; the explanation here follows Hill's interpretation. On the figure, Galileo has written numerals for all the horizontal intervals at the different vertical levels, all of which are reproduced by Hill (1988, pp. 647–648). According to Hill's calculations, the curves approach a parabolic form the farther they extend away from the table. Hill speculates that they were generated by rolling balls down inclines of various angles of inclination, with the balls then being allowed to drop through different vertical distances,

either to the floor or to a board set at some intermediate height between the floor and the table top. Hill identifies four different heights and three different angles of inclination used in the experiment. As the angle of inclination decreases, the curves approach semi-parabolic form. This would seem to suggest that horizontal projection after a roll (which cannot be achieved with this experimental setup) would yield the sought-after parabolic form.

5. This diagram is sketched by Galileo on fol. 175v of MS Gal. 72. It is reproduced by Naylor (1980, p. 558).

6. This is the famous diagram on fol. 116v of MS Gal. 72, which has been subjected to many analyses since Drake (1973) first called attention to it. On the folio Galileo lists the height of the table, 828 units, and also various heights of fall down an incline, namely, 300, 600, 800, 828, and 1000 units. Along the horizontal at the level of the floor he then records measurements of horizontal projection, writing the figures 800, 1172, 1328, 1340, and 1500. For the last four figures he then provides a second set of figures, namely, 1131, 1306, 1330, and 1460, presumably his calculations of what the distances should be if the 800 figure is taken as the baseline and one is attempting to show that successive heights of fall are in the same ratio as the squares of the distances of horizontal projection. Should this relationship be verified experimentally, one would have proof that the velocity of fall is directly proportional to the time of fall, the principle Galileo would use for his theorems on naturally accelerated motion in the *Two New Sciences*. The proof is sketched in Wallace (1981a, pp. 154–156).

7. Since this essay was first presented at Vico Equenze Colloquium in June of 1993, I have further stressed the historical importance of the demonstrative regress appropriated by Galileo from the Paduan Aristotelians via the Roman Jesuits. My more fully developed thesis is that the use of suppositions within the intermediate stage of the regress, particularly suppositions relating to experimentation and mathematical reasoning, is still relevant in the present day for understanding how controversies have arisen, and then been resolved, in the development of modern science. For the detailed development of this thesis, see Wallace (1996), especially chs. 8–10.

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ADDENDA

Article I

p. 114, line 26: For a reflection on Duhem's thesis in light of further research, and how it contrasts with the view of another historian of science, Alexandre Koyré, see Essay V in this volume.

p. 117, line 33: At the time I wrote this I assumed from his writing that Dullaert was a friar. There seems to be no evidence that supports this statement.

p. 119, note 6: The original version of this essay, which contains the Latin texts that were omitted in the earlier reprint, is included in this volume as Essay II.

p. 120, line 28: See Essay II, p. 401.

p. 121, line 1: In the place just cited in Essay II I report Soto's statements "as intuitive, without empirical proof of any kind", *ibid.* This is true as it stands, but it does not rule out the possibility that Soto knew, at the time he wrote this, of empirical evidence that might support it.

p. 121, note 9: This contribution has been reprinted as Essay VIII in William A. Wallace, *Galileo, the Jesuits and the Medieval Aristotle* (Hampshire: Variorum, 1991).

p. 122, note 10: The first of the works cited here appeared as Essay VI in the Variorum volume mentioned in the previous entry; the second appears as Essay IV in the present volume.

p. 123, line 6: All of the remaining essays in Part II of the present volume supply details that corroborate this statement.

p. 124, line 12: This evidence has now been published and is available as Essay IV in the present volume.

p. 127, line 15: A fuller account of this historical development is given in Essay XII of this volume.

Article II

p. 384, line 4: An abbreviated version of this essay, from which the Latin texts given in its footnotes were deleted, will be found in William A. Wallace, *Prelude to Galileo: Essays on Medieval and Sixteenth-Century Sources of Galileo's Thought* (Dordrecht Boston: D. Reidel, 1981), 91-109. The entire essay is reprinted here because the deleted texts are important for understanding the fuller array of Latin texts given in a subsequent publication, Essay III of this volume.

p. 385, line 2: For a fuller reflection on Koyré's views of Soto, as contrasted with those of Pierre Duhem, see Essay V in this volume.
p. 393, line 22: Here I identify both John Dullaert of Ghent and Paul of Venice as Augustinian friars. There is no doubt that Paul of Venice was an Augustinian friar, but I now doubt that Dullaert was, though he seems to have consistently followed Augustinian teachings in his works.
p. 399, note 58: Here I mention my intention to publish Soto's complete texts relating to falling bodies at a later date. This promissory note was unfulfilled for almost thirty years, but it was finally realized in 1997. See Essay III in this volume.
p. 401, line 5: Again, see Essay III.
p. 401, line 16: Regarding my expression, "without empirical proof of any kind," see the addendum to Essay I above, at p. 121, line 1.

Article III

p. 271, note 1: Reprinted as Essay II in this volume.
p. 271, note 3: The essay on "Late Sixteenth-Century Portuguese Manuscripts" is reprinted as Essay IV in this volume.
p. 295, note 92: Same as the previous notation.
p. 304, note 120: The essay on "Domingo de Soto and the Iberian Roots of Galileo's Science" is reprinted as Essay I in this volume.

Article IV

p. 678, note 6: "The Enigma of Domingo de Soto" essay is now reprinted in its entirety as Essay II in this volume. The earlier version contained in the author's *Prelude to Galileo* omits the Latin texts that are essential for understanding the texts presented in the Portuguese manuscripts.
p. 679, note 8: This essay is reprinted as Essay X in this volume.
p. 688, note 20: Same as the previous notation.
p. 697, note 28: This essay is now published and is reprinted as Essay I in this volume.

Article V

p. 239, line 33: The conference was entitled "New Perspectives on Galileo" and was held at Virginia Polytechnic Institute and State University in October 1975.
p. 248, line 13: This essay is reprinted as Essay II in this volume.
p. 259, line 25: Wallace, 1968, is reprinted as Essay II in this volume.
p. 259, line 44: Wallace, 1986, is reprinted as Essay VIII in this volume.

p. 260, line 1: Wallace, 1987a, has been reprinted as Essay VI in William A. Wallace, *Galileo, the Jesuits and the Medieval Aristotle* (Hampshire: Variorum, 1991).
p. 260, line 3: Wallace, 1987b, has been reprinted as Essay VIII in the book cited in the previous notation.

Article VI

p. 22, note 2: both essays cited in this note are reprinted in this volume, the first on "Iberian roots" as Essay I and the second on "'Laws' of Motion" as Essay III.
p. 22, note 11: The essay cited here is reprinted in its entirety as Essay II in this volume.
p. 22, note 13: The essay cited here is reprinted as Essay IV in this volume.
p. 24, note 30: Both essays cited in this note are reprinted in this volume, the first on "Regressive methodology" as Essay XIII and the second on "Circularity and the demonstrative regress" as Essay XII (...Paduan regress in this volume).

Article VII

p. 288, line 10: In my publications up to 1988 I have used Valla as the family name of this author, since this is the way his name appears in the *rotulus* of professors teaching at the Collegio Romano. Subsequently I have identified him as Paolo della Valle or Paulus Vallius, the latter being the name he himself used when publishing his *Logica* of 1622. This accords with an alternate name, de Valle, that appears on some of his manuscripts; it also serves to differentiate him from the many Valla's mentioned in the literature of the period. I wish to thank Paul Oskar Kristeller for suggesting this change to me.
p. 289, note 4: This essay has been reprinted as Essay X in the book cited above at V: p. 260, line 1.
p. 289, note 5: This essay has been reprinted as Essay IX in the book cited in the previous notation.
p. 289, note 6: This essay has been reprinted as Essay XII in the same volume.
p. 290, note 20: This essay has been reprinted as Essay II in the same volume.
p. 291, note 32: This essay has been reprinted as Essay III in the same volume.

Article VIII

p. 48, line 31: See the first notation for the previous essay at p. 288, line 10.
p. 54, note 33: This essay has been reprinted as Essay I in the book cited above at V: p. 260, line 1.

Article IX

p. 16, line 17: Since this was written the content of MS 27 has been published as Galileo Galilei, *Tractatio de praecognitionibus et praecognitis* and *Tractatio de demonstratione*, transcribed from Latin autograph by W.F. Edwards, with an introduction, notes, and commentary by W.A. Wallace (Padua: Editrice Antenore, 1988). An English translation of the work has also been published as William A. Wallace, *Galileo's Logical Treatises*, a translation, with notes and commentary, of his appropriated Latin questions on Aristotle's *Posterior Analytics* (Dordrecht-Boston-London: Kluwer Academic Publishers, 1992).
p. 17, line 39: see the first notation for Essay VII at p. 288, line 10.
p. 21, column 2, line 15: For *Secondua* read *Secunda.*
p. 34, line 28: Galileo's knowledge of demonstration is clearly exhibited in the works cited above at p. 16, line 17. A more systematic presentation of his teaching on, and use of, demonstrative processes is given in William A. Wallace, *Galileo's Logic of Discovery and Proof*, the background, content, and use of his appropriated treatises on Aristotle's *Posterior Analytics* (Dordrecht-Boston-London: Kluwer Academic Publishers, 1992).

Article X

p. 47, note 7: This essay is reprinted as Essay VIII in this volume.
p. 50, note 60: The word "contracts" on the last line of this note should read "contacts".
p. 50, note 61: The volume under preparation being referred to here actually appeared in 1992 as two volumes, the first cited in the notation for Essay IX at p. 16, line 17 and the second in the notation for the same essay at p. 34, line 28.

Article XI

p. 451*, line 31: Wallace, 1986a, appears as Essay VIII in this volume.
p. 452*, line 3: Wallace, 1990, appears as Essay X in this volume.
p. 452*, line 18: Wallace, 1993, appears as Essay XIV in this volume
p. 452*, line 21: Wallace, 1995, appears as Essay XII in this volume.

Article XII

p. 76, note 2: This essay has been reprinted as Essay V in the book cited above at V: p. 260, line 1.
p. 83, line 7: Since writing this I have been informed by Paul F. Grendler that Francesco Securo di Nardi died at Padua in 1489, where he had held the chair of

Thomistic metaphysics since 1464. Further details are give in Grendler's *The Universities of the Italian Renaissance* (Baltimore and London: The Johns Hopkins University Press, 2002), p. 386, n. 58.

p. 94, note 34: My essay on "Galileo's Sources" is reprinted as Essay VIII in this volume and my essay on "Dialectics, Experiments, and Mathematics" is reprinted here as Essay XIV.

Article XIII

p. 231, note 9: Two essays referred to in this note are reprinted in this volume: "The Enigma of Domingo de Soto" appears here as Essay II and "Domingo de Soto's 'Laws' of Motion" as Essay III.

p. 231, note 11: The essay "Domingo de Soto and the Iberian Roots" is reprinted in this volume as Essay I.

p. 233, line 11: The volume bears the title *Method and Order in Renaissance Philosophy of Nature: The Aristotelian Commentary Tradition*. As Professor Ekhard Kessler explains in his Introduction, pp. vii-xi, the volume itself was the result of a two-week postgraduate seminar he directed in Wolfenbüttel, Germany, entitled "Method in Sixteenth-Century Aristotelian Commentaries" (for details, see the Introduction to the volume). His own essay in the volume, entitled "Method in the Aristotelian Tradition: Taking a Second Look", pp. 113-142, was actually the key-note address that set the agenda for the seminar.

p. 233, note 18: The essay cited here is reprinted as Essay VIII in this volume.

p. 250, note 34: The first essay cited here appears as Essay I and the second as Essay V in this volume.

Article XIV

p. 123, line 44: Wallace, 1981b, has been reprinted as Essay III in the volume cited above at V: 260, 1.

p. 123, line 47: Wallace, 1983, has been reprinted as Essay II in the same volume.

p. 123, line 53: Wallace, 1987, has been reprinted as Essay VIII in the same volume.

p. 124, line 1: Wallace, 1988, has been reprinted as Essay V in the same volume.

p. 124, line 9: Wallace, 1995, is reprinted as Essay XII in the present volume.

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